Sci Bibli 8/24/59

SCIENCE

7 August 1959

Volume 130, Number 337

46566

Editorial Dither 301

Articles Stochastic Models of Population Dynamics: *J. Neyman* and *E. L. Scott* 303

A few simple chance mechanisms may combine to reproduce many manifestations of a complex phenomenon.

Radiocarbon from Nuclear Tests: *W. S. Broecker* and *A. Walton* 309

During the past 4 years man has been producing carbon-14 about 15 times faster than nature.

Emotion in Prejudice: *J. B. Cooper* 314

Physiological tests support the thesis that prejudicial attitudes are attended by relatively strong emotion.

William Fielding Ogburn, Social Scientist: *A. J. Jaffe* 319

News of Science Scientist Exchange Pact Signed by Nesmeyanov and Bronk; Nuclear Ship *Savannah* Launched; Seven NATO Allies To Get Atomic Data from U.S. 321

Book Reviews A. Koestler's *The Sleepwalkers,* reviewed by *M. K. Munitz*; other reviews 326

Reports Taxonomic "Descriptions": *R. D. Shenefelt* 331

Bomb Carbon-14 in Human Beings: *W. S. Broecker, A. Schulert, E. A. Olson* 331

Carrier State in Human Acatalasemia: *E. T. Nishimura* et al. 333

Presence of Myoglobin in "Cartilage" of the Marine Snail *Busycon: J. W. Lash* 334

Notes on the Champlain Sea Episode in the St. Lawrence Lowlands, Quebec: *J. Terasmae* 334

Growth Responses of Phycomyces to Polarized Light Stimuli: *W. Shropshire, Jr.* .. 336

On the Instrumental Conditioned Reaction Evoked by Electrical Stimulation of the Hypothalamus: *W. Wyrwicka, C. Dobrzecka, R. Tarnecki* 336

Isolation of Several Peptides with Relaxin Activity: *E. H. Frieden, N. R. Stone, N. W. Layman* 338

Departments Letters 298

Contamination of the Moon; Forthcoming Events; New Products 340

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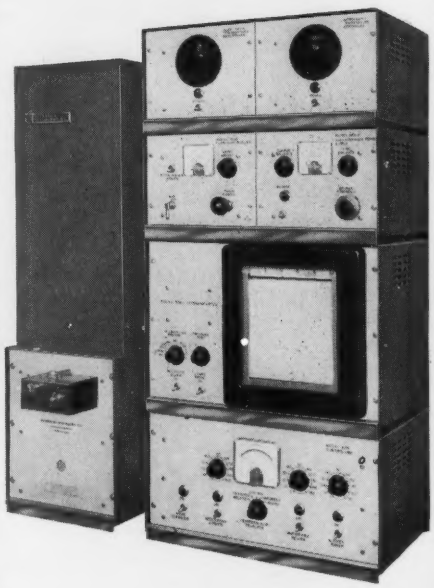

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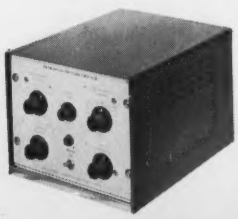

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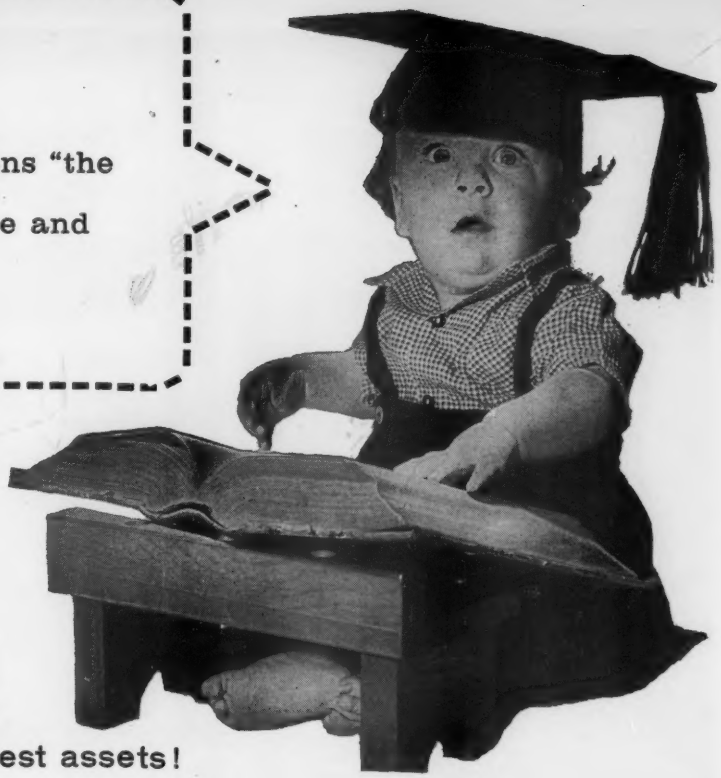

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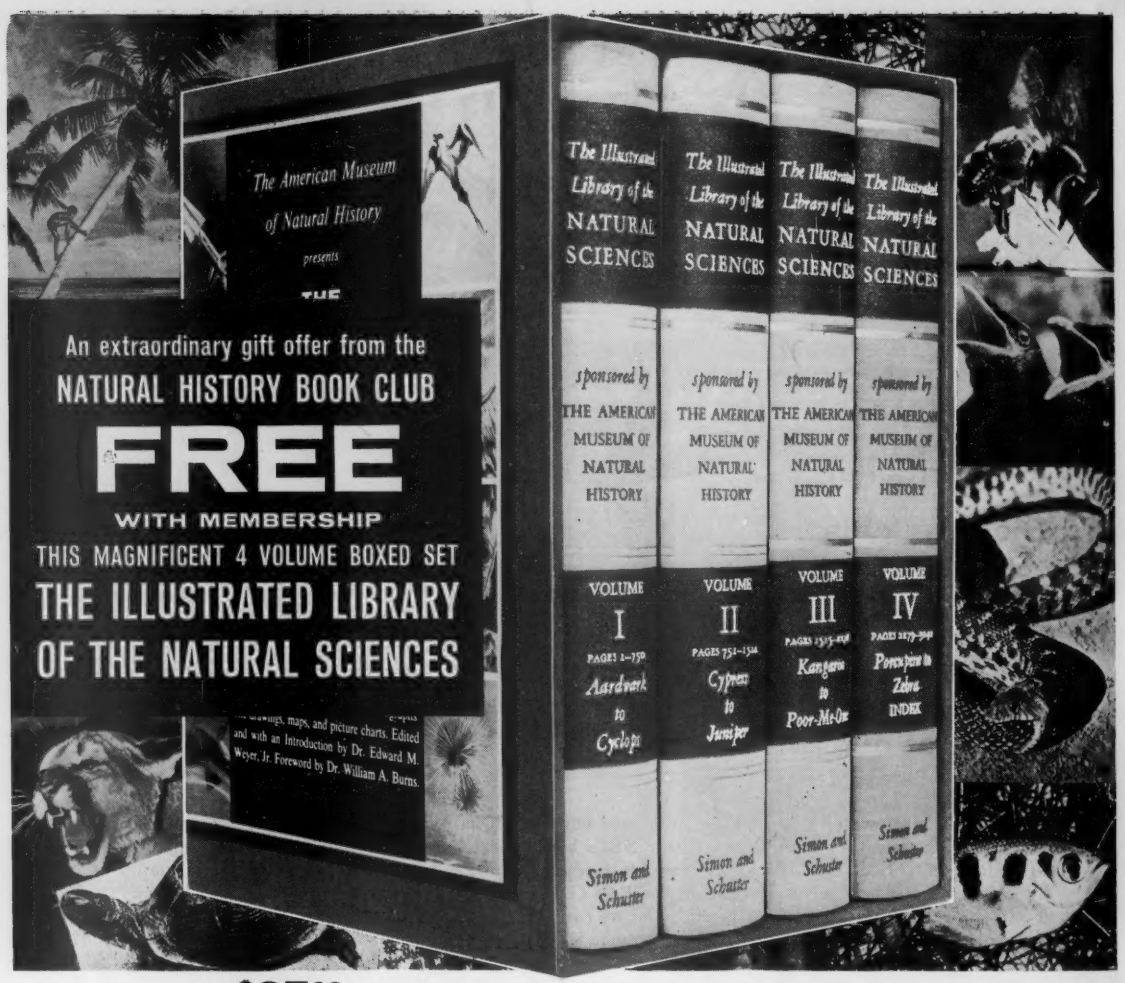
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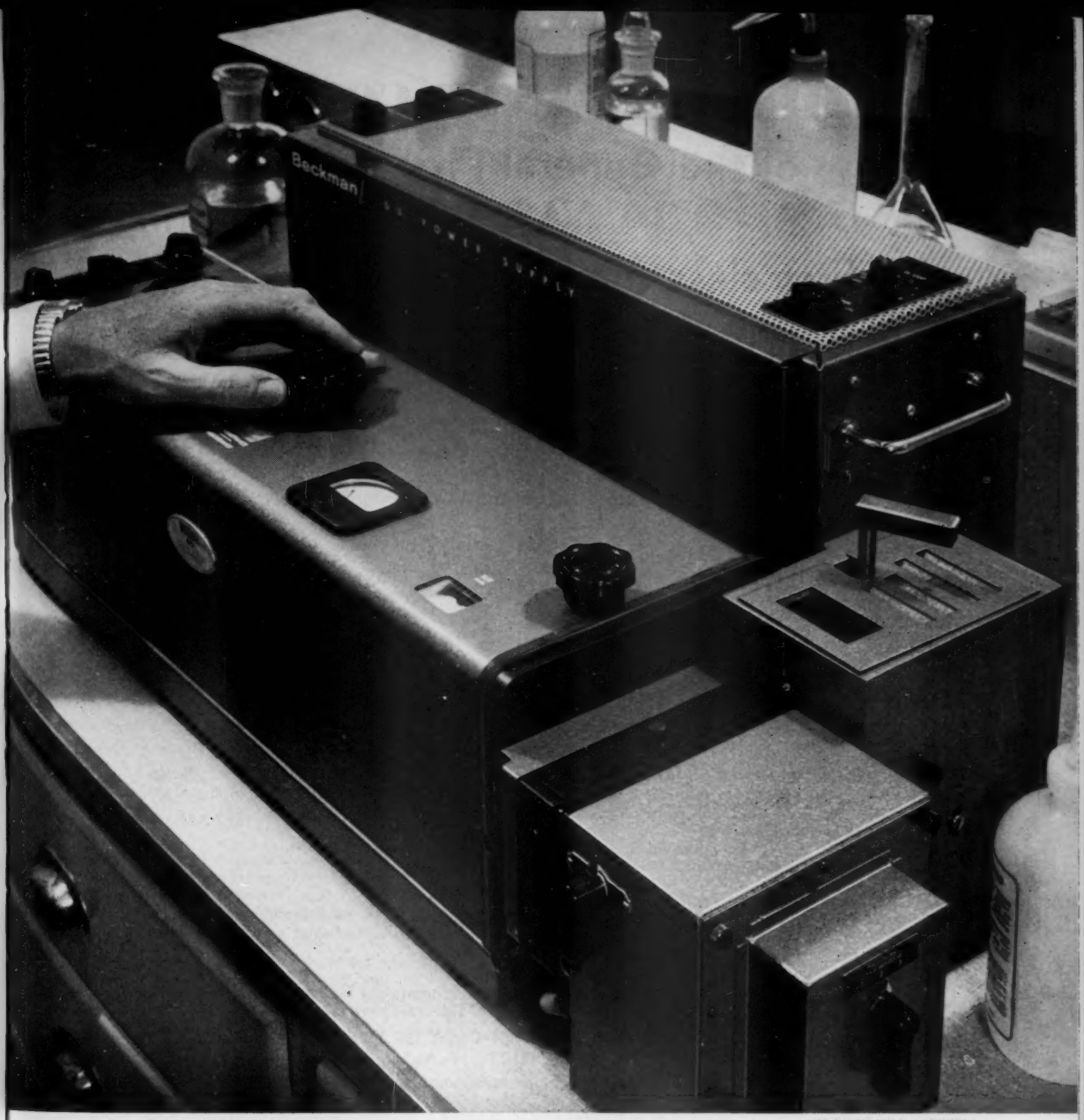

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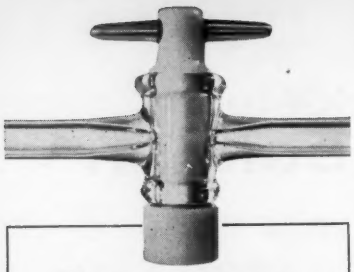

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Letters

On Supplementing Human Faculties

It was hardly to have been predicted that when a human being is completely isolated from external stimuli, as was the case in an experiment conducted by the Office of Naval Research in 1956 [discussed at the Interdisciplinary Conference on Self-Organizing Systems, Chicago, May 1959], he loses his sense of orientation and cannot change a set idea in his mind. Rather one would have assumed that he could continue to meditate on past experiences, much as if he were sitting alone in the dark of a warm, silent cave. The question occurs: Can a person be thrown into a higher level of consciousness by supplementation of his faculties? Certainly the effect of faculty supplementation can be observed in ourselves to a mild degree when we are listening to music or sitting in a Cinerama theater. What would happen if a more complete coupling were achieved between man and an artificial world, or between a person and more intense stimulation from his natural world?

This leads to another question: What faculties would have to be added to a monkey to make him react like a human being? For one thing, a monkey lacks the ability to formulate and recognize symbols. But he can learn a great deal. Conceivably, an electronic aid to symbol recognition or formulation might be provided for one of our simian friends, and he could be conditioned to use it, say, to recognize and indicate the letters of the alphabet. How much aid in grammar formulation and word categorization would we have to provide him (by means of a computer, perhaps) before he could begin to talk sensibly to us? Would this aid be less than that required by a computer operating alone?

The crux of the matter is undoubtedly in the coupling between external devices and animals. A device has to be more perfectly woven into our nervous system before we can begin to tap it as we naturally tap our other faculties. Also, some control, perhaps even a random control, must be provided, because if the extrasensing devices were not controllable they might assume the upper hand. Of course, in a movie we can shut our eyes, but more control would be necessary with extrasensing devices. If an electronic memory could be coupled into our brain we would have to have some means of scanning it in order to get at arbitrary information.

Before coupling of any higher degree of sophistication than that achieved in a stereophonic Cinerama theater can be achieved, a more thorough understanding must be had of the coding of signals in the brain. Yet, if a person (or a monkey) were conditioned to substitute senses (as, for instance, if supersonic tones in a spectrum of frequencies were transduced into pinpricks along the spine) or coupled to a more perfect form of Cinerama, would he react normally under this greater degree of stimulation?

These speculations may perhaps seem fantastic, but in the light of the results of the Office of Naval Research experiment they must be given serious consideration.

ROBERT E. MUELLER
Astro-Electronic Products Division, Radio Corporation of America, Princeton, New Jersey

Phosphorus and Phytoplankton

In the article "Bound phosphorus and growth of phytoplankton," by Whitford and Phillips [*Science* **129**, 961 (1959)], the implication that phosphorus was not limiting because total phosphorus showed no correlation with phytoplankton pulses does not appear to be valid. Not only do the authors point out that total phosphorus varied with the rainfall but they acknowledge that the phytoplankton populations were low (and presumably, therefore, would require a relatively small amount of phosphorus). Also, and more significant, total phosphorus includes a galaxy of compounds, particulate and soluble, organic and inorganic, of which the only fraction now unequivocally known to be utilizable by algae is soluble orthophosphate. This fraction generally accounts for only about 10 percent of the total phosphorus in lakes, and even in the attempt to correlate this fraction with phytoplankton, the problem is complicated by the fact that certain plankton algae are known to be able to store up phosphate under conditions of plenty and to use this reserve for growth in times of phosphate deficiency. Therefore, any correlations observed between total phosphorus and phytoplankton are likely to be fortuitous, and any lack of correlation is likely to be without significance.

JOSEPH SHAPIRO
Department of Zoology, University of Washington, Seattle

The criticism by Shapiro of our report is probably justified on the basis of data given. We believe, however, that our conclusions, which were based on data not adequately presented in the article, are valid. We are aware of the number and complexity of phosphorus compounds in natural aquatic habitats. Our analyses of total phosphorus were

(*Continued on page 347*)

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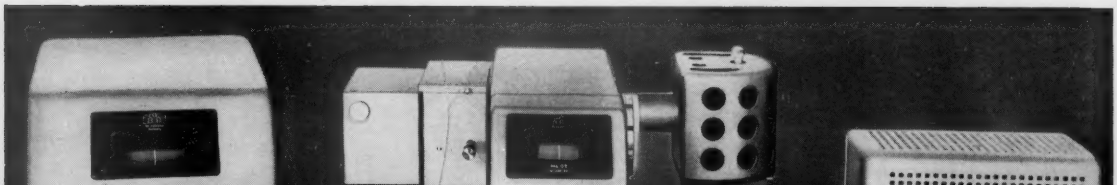

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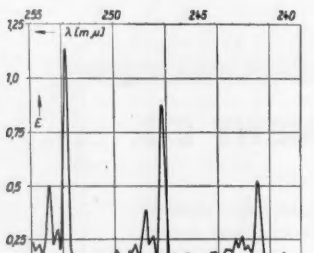

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Dither

When working, early in World War II, on antiaircraft computing and gun-laying devices, I learned from British colleagues a new use of an old word. When building such devices, they often included a small eccentric or vibrating member which kept the whole mechanism in a constant state of minor but rapid vibration. This they called the "dither."

The purpose, once one thinks a moment, is perfectly clear. Kinetic friction is less than static friction. If the parts are constantly in slight motion, then the whole device is alert, is on the jump, is ready to respond promptly to the earliest beginnings of forces seeking to move the gun, the little dither gimmick preventing any sluggish delay caused by static friction. The same phenomenon arises in other connections. For example, the types of flight instruments which are dependably responsive in an aircraft with reciprocating engines and a good deal of resulting general vibration might tend to stick or respond tardily in jet aircraft with their almost vibrationless flight.

We need a certain amount of dither in our mental mechanisms. We need to have our ideas jostled about a bit so that we do not become intellectually sluggish. The British are good at this, too, and use Hyde Park corner, His Majesty's Loyal Opposition, and a variety of other techniques to make sure that mental static friction does not dominate.

It is a wonderful thing to have in science fresh, unorthodox, nimble, and vibrating minds. The recent phenomenon of Edwin Land's essentially 17th-century type of discovery in the field of color vision is an excellent case in point. This really shakes every scientist, and in so doing it benefits him.

The same consideration, I think, furnishes an extremely powerful argument for carrying out as much basic research as possible under circumstances which involve eager young persons. My scientific godfather, Dean Charles Sumner Slichter of the University of Wisconsin, used to say that every laboratory ought to have "one damn fool who doesn't know yet what you can't do." When a professor who does research also has the marvellous opportunity of teaching elementary courses he finds that the contacts with the youngsters constantly jostles his own mind. It may at times put him in a dither, but that is good for him.—WARREN WEAVER, *Alfred P. Sloan Foundation, New York.*

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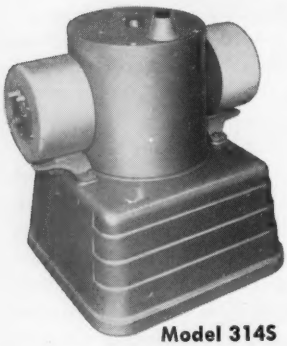

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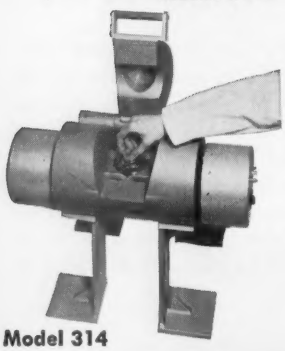

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Stochastic Models of Population Dynamics

A few simple chance mechanisms may combine to reproduce many manifestations of a complex phenomenon.

Jerzy Neyman and Elizabeth L. Scott

Before proceeding to the main subject of this article, it may be useful to indicate briefly the meaning which we ascribe to the terms *model* and, in particular, *stochastic model*. The question is, what do we mean when we say that we build a stochastic model of a given class of phenomena?

In considering this question, it is necessary to remember that, ordinarily, phenomena of interest are not open to immediate observation in all of their details. Most frequently, we observe only a few external manifestations of the phenomena. The figures on births and deaths, employment and unemployment, and so forth, collected by the Bureau of the Census exhibit several manifestations of the complex phenomenon designated by the phrase "the dynamics of the population of the United States." Again, the observations of Thomas Park represent an interesting manifestation of a complex phenomenon of struggle for existence. These observations refer to two noninterbreeding and nonaggressive species of flour beetles. If these species are forced to share the same habitat, then one, not always the same, invariably becomes extinct. On the other hand, and this is another manifestation of the same phenomenon, each species, if kept separately in the same kind of habitat, is capable of maintaining a considerable population, the size of which fluctuates about a point of apparent equilibrium.

In each of the two cases, the dynamics of the population of the United States and the struggle for existence of flour beetles, there are a variety of observations yielding a number of different manifestations of a complex phenomenon. However, the complete picture of the phenomenon remains hidden.

Another element to bear in mind while approaching the term *model* is the existence, for each of us, of a certain category of phenomena, or mechanisms, which, partly through experience and partly through imagination, appear to us familiar and, therefore, simple. Although, in some cases, this simplicity is only apparent and our familiarity not very deep, we are inclined to believe the contrary and to think in terms of these familiar mechanisms. For this particular reason, we shall call these mechanisms "elementary" mechanisms.

A classical example of an elementary chance mechanism, or chance phenomenon, is the stability of the frequency of "heads" in a series of tosses of a coin. Although not many of us have done any extensive coin tossing in our life, such experience as we do have, combined with imagination, make this particular mechanism a very familiar one.

With these two prerequisite concepts, (i) the essentially hidden complex phenomena of which we observe only certain manifestations and (ii) simple mechanisms, which are broadly familiar through experience and imagination, we may now explain the sense in which we use the terms *mathematical model* and *stochastic model*.

When we speak of a mathematical model $M(\Pi)$ of a class of phenomena Π we mean a system of mathematically expressed hypotheses representing Π as a particular combination of several elementary mechanisms. These mechanisms may be either deterministic or indeterministic. If a particular mechanism implies that every variable of the system contemplated is a function of the others, then this mechanism is called deterministic. Otherwise, that is, if at least one of the variables is a chance or a random variable, then the mechanism is indeterministic. The word *stochastic* is used as a synonym of *indeterministic*.

As a consequence of the above explanations, a stochastic model of population dynamics means a system of mathematically expressed hypotheses, representing the development of a population as a particular combination of elementary mechanisms, at least some of which are chance mechanisms.

At this point one might wonder about a distinction between mechanisms which are and which are not "elementary." As we see it, the distinction is both simple and subjective. Mechanisms with which through personal experience and imagination we feel familiar are "elementary" to us but, perhaps, not to others. Also, there are, undoubtedly, a great number of chance mechanisms familiar to physicists to the point that they are able to think in terms of these mechanisms, which are entirely unknown to us. These mechanisms, then, are "elementary" for the physicists but not elementary for us. In general, the greater the number of elementary chance mechanisms that one has at his disposal, the easier it is to construct stochastic models of phenomena.

Both authors are on the staff of the University of California, Berkeley. Currently, Professor Neyman is in the Miller Institute for Basic Research in Science. This article is based on a paper presented on 29 Dec. 1958 at the Washington meeting of the AAAS, when the authors were awarded the Newcomb Cleveland Prize. It was presented at the invitation of the Society for General Systems Research, the principal aim of which is to encourage the development of theoretical systems applicable to more than one of the traditional departments of knowledge.

Purpose of Models

We see two distinct purposes of stochastic models, one esthetic and the other utilitarian.

When it happens that a number of apparently disconnected manifestations of a class of phenomena are logical consequences of a single simple combination of a few "elementary" mechanisms then, to most of us, this circumstance is a cause of distinct esthetic satisfaction. This satisfaction is particularly strong if, after studying the conceptual model, we suddenly notice an observable detail which thus far escaped the attention of the experimenter and on which the predictions of the theory appear to agree with empirical fact. Upon observing a circumstance of this kind we generally feel that we "understand" the development of the phenomena.

The utilitarian considerations regarding models come in when we are concerned, for practical purposes, with such manifestations of a particular class of phenomena which, momentarily, are not directly observable. At that moment, the availability of a model, whose consequences agree with a number of manifestations of the same phenomena already observed, is most convenient because it provides suggestions about what might be expected in the domain where the observations are still lacking. A case in point is the present discussion, in the scientific literature and the daily press, whether the increase in radioactivity caused by the tests of nuclear weapons is or is not going to increase the incidence of cancer among human beings. Cancer develops slowly, and direct empirical evidence will require years to accumulate. Thus, in order to have a hint right now about what might be the effects observable in the future, our only source is a theoretical model of the phenomenon of carcinogenesis. If one is constructed and if it stands up in comparisons with a number of different manifestations of the phenomenon already observed, then, and until direct observations of the effects of increased irradiation are available, the predictions of the model will form a natural basis for practical action.

Subject of This Article

In the sections that follow we attempt to give a brief account of three distinct stochastic models of dynamic population phenomena with which we have been concerned in our work. Details of two of these models have already been published, and our present purpose is to analyze the solutions obtained in terms of the concepts of "elementary mechanisms" mentioned at the outset. These two models are: a model (*1*) of struggle for existence between two nonaggressive, noninterbreeding species (*2*) and a model of clustering which appears to have interesting applications in a number of domains, from the temporal-spatial development of biological populations (*3*), through physics (*4*) to cosmology (*5*). The third model (*6*) is concerned with the temporal development of populations of cancer cells (clones) conceived as generated by a two-step, mutation-like process. Due to limitations of space, publication of this model is postponed.

Struggle for Existence

Thomas Park's observations on the two species of flour beetles, *Tribolium confusum* and *Tribolium castaneum* (denoted by him *b* and *c*, for short), extend over a very considerable set of experimental conditions. We are concerned here with one set of experiments, in a particular combination of temperature and humidity, where the species *b* and *c*, kept separately from each other, were found to be capable of maintaining relatively stable populations indefinitely. The numbers of adults, counted once a month, varied about averages which, themselves, varied from one replicate of the experiment to the next. For species *b* the averages varied from about 70 to 130, the grand average being about 110. For species *c* this variation was from 80 to 220, with the grand mean about 140. Thus, on the average, species *c* appeared to be more "viable" than *b*.

The interesting feature of the experiment was that in all vials containing a combined culture of both species *b* and *c*, initially in equal numbers, one of the species invariably died out completely. The further interesting detail is (i) that the dying-out species was not always the same and, particularly, (ii) that the more frequently dying-out species was species *c* which, in separation from the other, is capable of maintaining a larger population!

The basic question here was whether this surprising result of the experiment is indicative of some *biological* changes in the beetles of the two species induced by cohabitation or whether it is a natural outcome of the interplay of their biological functions exhibited equally when the two species live in isolation and when they share the same habitat. When speaking of biological changes, we have in mind possible changes in the fecundity, fertility, and duration of the several phases in the life cycle that might conceivably occur in one (or both) species, perhaps due to the contamination of the flour by the excreta of the other species, and the like.

Our contribution to the study was very modest. We simply put in formulae the facts found by Thomas Park and recounted to us. These facts are:

1) While the beetles of the two noninterbreeding species have no racial prejudices, and do not fight, they do consume each other's eggs and pupae. Also, it appears that in this particular respect there are again no prejudices and each of the species likes the eggs and the pupae of the other just as well as its own.

2) In general, no two females lay their eggs with the same abundance but, in addition, there appears to be a difference between the two species.

3) There appear to be variations in the egg and pupae eating both within and between species.

4) Similarly, there appears to be a within- and between-species variation in the duration of inactive forms, egg and pupae.

These four characteristics, observed for the two species reared in isolation from each other, were taken as the basis of the stochastic model constructed. For the sake of simplicity (in fact, we were not able to do otherwise) the model constructed is of the type called a discrete time model. Also, we ignored the distinction between sexes. The basic idea was to compute for each egg the chance, depending upon the number of beetles present in the vial, of surviving to hatch. Naturally, given a fixed number of predator forms, the longer the egg stage, the smaller the computable probability that the egg will hatch and, eventually, produce an adult.

The consequences of these hypotheses proved to be in qualitative agreement with the observations both of development in pure cultures of the two species and in competition. Specifically, with reference to pure cultures, it was shown that, in order to explain the time fluctuations of the number of adults about a point of apparent equilibrium, there is no need to postulate any *biological* changes due to crowding. The phenomenon is a simple consequence of the assumptions that the fecundity and fertility are random variables with fixed distributions, that the duration τ in the

inactive states (egg and pupa) is a constant, that for each egg the probability of being eaten by a given beetle during a given period of time has a fixed value, independent of crowding, and that the fates of particular eggs and of particular beetles are mutually independent.

With reference to competition of two species, it was also shown that the invariable extinction of one of them does not require any assumption of biological changes induced by cohabitation or by crowding. In fact, the necessity of extinction of one of the two species is a consequence of the assumption that the two species in competition have the same characteristics as in separate cultures and that they differ in the value of the criterion

$$C = (\log \nu)/\tau$$

where ν stands for the global-fecundity, meaning the average number of adults generated by a female during her lifetime in the absence of cannibalism. According to the model, of the two competing species the predominant winner of competition is the one with the larger value of the criterion C.

The question arises whether the above success of the model *proves* that crowding and cohabitation induce no biological changes in the beetles. The answer is no. All that is shown is that the observations described do not constitute evidence for the existence of such changes. In fact, some further, more delicate experiments may show that because of crowding or because of cohabitation with another species, the biological properties of the beetles do change.

Further developments regarding the model may be of interest, and we wish to report the following two items. First, a paper by Amelia Polnik (*7*) showed that the factors of unequal fecundity and unequal duration in inactive states certainly play an important role in the competition of species. Second, according to a personal communication of Thomas Park, the original model of competition described above appears incomplete. In fact, further experiments with the two species of *Tribolium* revealed two genetic strains of one of the species which have practically identical values of the criterion C. According to the original model, the fates of these two strains in competition with the one strain of the other species should be the same. Yet, in actual experiments, one of the strains is a consistent winner and the other a consistent loser in competition with the same opponent. Thus, the two strains must differ in some important characteristic not involved in the original model. A preliminary inspection of experimental data already obtained suggested to Thomas Park that this relevant characteristic is the total duration of the life cycle "from egg to egg." The shorter this period, then, *caeteris paribus,* the better the chance of winning the competition. We are looking forward to the completion of this novel series of Park's experiments, which we expect to yield further important information.

Stochastic Process of Clustering

The second stochastic model of population dynamics to which we wish to call attention is the so-called stochastic process of clustering. The first idea of this process occurred to one of us (*8*) in 1939 in connection with the distribution of the number of insects in equal-sized plots on the ground. Subsequently, this same chance mechanism proved useful during the war in the theory of bombing by formations of planes. After the war, we found that the same process, unexpectedly, has interesting applications in indeterministic cosmology (*9, 10*), concerned with the phenomenon of clustering of galaxies. Also, one of our colleagues, Robert R. Read, found interesting applications of the clustering process in the study of ionization in a cloud chamber caused by cosmic rays. Still more recently (*3*), armed with the same stochastic process of clustering, we returned to the study of biological populations. Finally, we now have hopes of obtaining some interesting results in applying the clustering process to carcinogenesis.

In addition to this multiplicity of applications, the process of clustering is very interesting because many of its properties still remain unexplored and because there is a broad field for mathematical generalizations.

The particular novel elements in the stochastic approach to the study of populations introduced by the process of clustering are two.

1) Practically all classical attempts to study chance mechanisms of the development of a population treat all the individuals forming the population, in a sense, uniformly. Striking examples of this "uniformity" are the various theories of epidemics. Here it is usually assumed that an increase in the number of infectious individuals increases equally the probability of becoming infected for every single susceptible in the population. As the relevant authors themselves point out, this equality in the increase of the probability of infection may be realistic only in very small communities, such as families. On the other hand, while a case of bubonic plague in a community does indeed increase the danger of infection with this disease everywhere, the increase in the household of the individual concerned must be presumed to be greater than that for a typical person living in a distant city.

The first novel element introduced by the stochastic process of clustering is that here the populations studied are not treated as aggregates of individuals all characterized by the same probabilities but as aggregates of groups or aggregates of clusters.

2) Another element of the classical approach to population dynamics is that the two aspects observed in the development of populations, namely (i) migration and (ii) changes in the numbers of individuals forming the population, are treated separately. The usual mathematical model of migration is the so-called diffusion process. The subject of study is the aggregate of chance motions either of one particle starting from a given point or of a number of such particles. The mathematical instruments of study of numerical changes in a population are the so-called multiplicative stochastic processes, concerned with variation in time of the number of individuals, under certain hypotheses about immigration, birth, and death. In other words, the classical diffusion processes are concerned with the time and space coordinates of some existing particles but ignore the phenomena of their birth and death, while multiplicative processes deal with births, deaths, and immigration but ignore the spatial relationships of the particles.

The second novel element introduced by the process of clustering is that in its treatment of populations it provides a unified theory of spatial as well as of numerical changes in the population. Specifically, for any preassigned system of "regions" in the habitat, say

$$R_1, R_2, \ldots, R_s$$

and for any epoch t, the stochastic process of clustering studies the joint distribution of $2s \times n$ variables

$$\gamma_{jk}(R_i, t)$$

with $i = 1,2, \ldots, s$; $j = 1,2$; and $k = 1,2, \ldots, n$. Here

$$\gamma_{1k}(R_i, t)$$

represents the number of male members

of the kth generation of the population which at time t are alive and are located within the region R_i. Further,

$$\gamma_{sk}(R_i, t)$$

represents the number of females of the same kth generation alive at time t and, at that time, located in R_i.

Thus the process of clustering represents a particular combination of two mechanisms that one might consider "elementary" (although each of them is rather difficult): the diffusion mechanism and the birth and death mechanism, both stochastic mechanisms. Since these two "elementary" mechanisms are still the subjects of intensive mathematical research, it must be obvious that the possibility of their combination is limited to considerable simplifications and that whatever results are obtained now have been obtained at some cost. There are several items in this cost that we sincerely regret. One is our inability to treat the problem other than by assuming that multiplication processes occur only at fixed intervals of time.

Postulates Underlying Clustering

After this somewhat long introduction, we enumerate briefly the postulates underlying the process of clustering. These postulates may be stated in terms of abstract "particles" which later can be identified with more or less arbitrary concrete entities, such as cosmic ray particles, galaxies, beetles, cancer cells, or even ideas. However, we expect a gain in intuitiveness by speaking directly of consecutive generations of some living species.

We begin by contemplating a habitat, essentially a Euclidean space of an arbitrary number of dimensions, possibly a plane. In this habitat, we introduce a system of coordinates and consider arbitrary regions $R_1, R_2, \ldots$, postulated to be Borel-measurable. The datum of the problem is the distribution law of "ancestors" of a population. Given an arbitrary finite system of regions

$$R_1, R_2 \ldots, R_s$$

this law must determine the joint probability distribution of the random variables $\alpha(R_i)$ and $\beta(R_i)$, $i = 1,2, \ldots, s$, representing the numbers of male and female ancestors which at time $t = 0$ are located in R_i. For example, at time $t = 0$ there may be just one female ancestor located at the origin of coordinates. Then

$$P\{\alpha(R_i) = 0\} = 1$$

for all i, while

$$P\{\beta(R_i) = 1\} = 1$$

only if the region R_i contains the origin of coordinates. Alternatively, it may be given that at time $t = 0$ the ancestors, both males and females, are Poisson-wise distributed over the habitat (or over a specified part of it), with a constant or a variable density, and so forth.

Further postulates are as follows.

1) *Postulate regarding births.* It is assumed that at each moment kT, where T is a fixed positive number and $k = 0,1, \ldots$ every then living female, whether of the ancestral or of any other generation, gives birth to a "litter" of progeny. The individuals born at moment kT form the kth "generation."

2) *Postulate regarding the distribution of litters.* The point at which a litter is born coincides with the location, at that time, of the mother female.

3) *Postulate regarding the size of litters.* The number of members of a litter is a random variable $\nu = 0,1,2, \ldots$, with an unspecified probability generating function $G_\nu(u)$ which may be assumed to depend on the generation of the mother female or on the location of the litter, or on both. However, it is assumed that the variables ν corresponding to different litters are mutually independent and independent of all other variables involved in the model.

This is one of the assumptions that we were forced to adopt with great regret. One of the notable features in the development of real populations is the possible dependence of the birth rate on the degree of local congestion of the population so that an important generalization needed in the theory of clustering is a device, seemingly easy but one we found difficult, introducing "density dependent" distributions of the size of the litter.

4) *Randomness of sex.* It is assumed that the determination of sex of a litter member is random, with a fixed probability F that a given individual will be a female. The determinations of sex of any given individuals are mutually independent and independent of all other variables of the system.

5) *Random survival.* For any two positive numbers t_1 and $t_2 > t_1$, we postulate the existence of the conditional probability $\theta(t_1, t_2)$ that an individual of the population alive at age t_1 will survive up to age t_2. For

$$0 < t_1 < t_2 < t_3$$

this probability must satisfy the consistency relation

$$\theta(t_1, t_3) = \theta(t_1, t_2)\theta(t_2, t_3)$$

The survival of particular individuals of the population is assumed to be independent of the survival of other individuals and independent of all other events in the system.

The probability of survival $\theta(t_1, t_2)$ may be assumed to depend upon the sex of the individual concerned and also on its location at its age t_1. However, we did not succeed in developing a theory involving "density dependence" of survival.

6) *Random migration.* The essential contents of this postulate are that the individuals of the population migrate in the same manner as, and independently from, one another and that the speed of migration depends only on the age of the individuals concerned. Mathematically, the postulate refers to an individual aged t_1 located at a point u (we use just one letter to denote the several coordinates of a point in the habitat, whether it is a plane or a higher dimensional Euclidean space), and to a subsequent moment of time $t_2 > t_1$. Assuming that the individual concerned survived up to age t_2, we denote by X its position at that time. X is considered a random variable with probability density

$$f(x - u, t_1, t_2)$$

depending upon the distance $x - u$ between the location x at age t_2 and the location u at age t_1, and on the ages t_1 and t_2. Also it is assumed that the migrations of particular individuals are mutually independent and independent of all other variables of the system. The function f thus introduced may be called the "dispersal" function of the litters. It must satisfy the obvious consistency relation.

Starting with the above postulates, we were able to obtain an explicit formula determining the joint spatial distribution of males and females of each two consecutive generations that are alive at any preassigned moment t. Also, a recursive procedure was developed leading to a similar result for any number of generations. Unfortunately, the process of births, the determination of sex, migration, and deaths are complicated, so that

the resulting formulae are also complicated and the limitations of time prevent us from writing the formulae. They have already been published (*3*).

Biological Populations

Out of the various domains in which the process of clustering has been applied thus far, the development of biological populations appears to be more complicated and more demanding than others. Here the main complicating factor is the multiplicity of generations contributing to the population of individuals alive at any time t. Also, there is the already mentioned demand for density-dependent probabilities. In other domains we frequently have to deal with only two generations, the "ancestors" and the "progeny." Furthermore, there is ordinarily, no embarrassment with the assumption of regularly spaced "births."

Bombing

In the study of bombing we were concerned with the so-called force requirement and the optimum method of releasing bombs. The criterion was the expected pattern of bombs over a target. Here the generation of "ancestors" is composed of imaginary points representing "centers of impact" of bombs released by single formations of planes. Their distribution is generated by the so-called "errors of aiming." The "progeny" are the single bombs, and their "migration" is the ballistic dispersion combined with the intentional spread achieved by the method of release of bombs by particular planes.

Cloud Chambers

In the problem of ionization within a cloud chamber, the "ancestors" are the so-called "primary events" consisting of encounters of the cosmic ray particle with the atoms of gas filling the chamber. These primary events are supposed to be Poisson-distributed along the path of the particle. Single primary events are not observable. On the other hand, the individual members of "progeny" "surviving" up to a certain time are observable, and the problem consists of deducing their distribution.

Each primary event creates a random number ν of pairs of ions. Also, most of the ions (again a random number) become centers of droplets. The ions created by a given primary event are its "progeny." Those of the ions that bear droplets are the "surviving" progeny. The "dispersal" of progeny corresponds to the displacements of droplets due to Brownian motion occurring between the time of passage of the cosmic ray particle and the time a photograph of the cloud chamber is taken. The observations give the numbers of droplets in unit lengths along the path of the cosmic ray particle, counted on the photographs.

Cosmology

The two problems just described are mentioned here not because of their intrinsic importance or particular interest but merely as illustrations, with the hope that their perusal may suggest other fields where the theory of the same process of clustering might be useful.

The domain in which the stochastic process of clustering appears to promise results more interesting than in others is the domain of cosmology. Here its applications, combined with unfortunately all-too-heavy numerical work, promise solutions of several problems of prime general interest which, as far as we can see, appear intractable in any other way. In fact, with a little luck in attracting the attention of more workers in the field, the process of clustering, with its further theoretical developments, may easily become the basis of a new theory which we like to call indeterministic cosmology. The presentation of the problems involved, in order to be intelligible, requires a substantial substantive astronomical background which may be found in a recent publication (*10*). Here we have to limit ourselves to a few brief remarks.

One of the most remarkable features of modern cosmology is a sharp divergence between the relevant mathematical work on the subject and the work of observational astronomers. The latter is concerned with single galaxies and with clusters of galaxies. The clusters appear as independent dynamical systems composed of a varying number of members. The variation in the number of cluster members is tremendous. There are galaxies which appear to be isolated, thus forming "clusters" with one single member. Also, there are known "double galaxies," or clusters with two members. Then there are groups of three, four five, and so forth, and, finally, there are clusters with memberships running into thousands. The member galaxies of particular clusters vary considerably in their structure, intrinsic brightness, and other characteristics. All this is the subject of intimate and intensive study by observational astronomers. Briefly, the subject may be described as the local concentrations of matter in our space, with their infinite variations of characteristics, mass, brightness, velocity, chemical composition, and so forth.

Paradoxically, while the observational astronomers concentrate their efforts on these local irregular concentrations of matter, the theoretical cosmologists begin with the hypothesis that no such local concentrations exist! In the words of one of the most distinguished contemporary theoretical cosmologists, all "the observed agglomeration of matter into stars and nebulae (galaxies), and even into clusters of nebulae, is to be replaced by a smeared-out substratum (a sort of fluid) which preserves only the uniformities common to all regions —and thereby robs it of most of those characteristics which make the skies a delight and a challenge to poet and astronomer alike!"

The reason for this divergence between observational and theoretical cosmology is that the latter is built on deterministic mathematics. In order to be able to treat the universe deterministically, it is necessary to deal with only a few hypothetical quantities characterizing, hypothetically, this universe, so as to be able to write down and to analyze the relevant differential equations. The results of this analysis are described as the "basic undergarment" of cosmology, and the hope is expressed that comparability with observations may be achieved by merely "tacking" onto the undergarment a few "frills and furbelows required to express individuality."

In addition to the concept of substratum, the classical cosmological theories adopt another hypothesis, described as the cosmological principle. The formulations of this principle are invariably extremely vague. Here is one of them: "Observations . . . have given rise to the notion that the distribution and motion of matter in any sufficiently large spatial region of this universe are, by and large, intrinsically much the same as those in any other similar region, regardless of its position and orientation. This presumed uniformity in the large

implies a certain form of a principle of relativity, sometimes called, appropriately enough, the 'cosmological principle.' "

There is little doubt that the "cosmological principle" is an extension of the Copernican revolution against the dogma of the earth's being the center of the universe. After denying this privilege to our planet, a further step was taken leading to the assumption that the universe has no "center" at all and that, in fact, "essentially," whatever happens in one spot of the universe, is "essentially" equivalent to whatever happens in another. While the idea itself is attractive, the notorious "local irregularities" in the distribution of matter jump to the eye and create embarrassment.

As far as we can see, this embarassment persists only so long as one searches among the concepts of deterministic mathematics for a system that can account for the "essential" uniformity of the universe combined with the observed tremendous "local irregularities." On the other hand, it seems to us that the difficulty immediately disappears if one adopts the indeterministic point of view. This involves the adoption of the attitude that the universe, as we see it, is a single realization of a four-dimensional stochastic process (three spatial coordinates and one time coordinate). The vaguely expressed assumption that "the distribution and motion of matter in sufficiently large spatial regions are, by and large, intrinsically much the same . . ." corresponds to a conceptually rigorous hypothesis that the stochastic process in question is stationary in the three spatial coordinates. Operationally, the same assumption of stationarity may be expressed by postulating that in every region of space there exists a particular chance mechanism, the same for all regions, governing the distribution of matter and its motions.

It must be obvious that the adoption of the point of view just described removes the cleavage between observational and theoretical cosmology. The "local irregularities" are no longer in conflict with the theory postulating universal uniformity. The reason is that the sameness of chance mechanisms operating at two different points in space or time, or both, is, by virtue of their being *chance mechanisms,* perfectly consistent with even the most marked difference in observable manifestations.

Considerations of this kind led us to believe that the esthetic value of the cosmological theory will be markedly increased if, instead of starting deterministically and then "tacking on" the frills and furbelows of indeterministic elements, indeterminism is adopted from the very beginning. The work involved consists of the invention of a four-dimensional stochastic process with consequences which would agree with observations. In constructing this process it is natural to take into account both the ideas underlying the classical deterministic theories and the concepts and preoccupations of observational astronomy. As already mentioned, the latter involve clusters of galaxies with varying numbers of members. Thus, inspired by discussions and joint work with colleagues C. D. Shane and more recently N. U. Mayall, both of the Lick Observatory, our own efforts over the last decade have been directed toward the construction of a four-dimensional stochastic process of clustering to represent the universe, and the development of methods of its study using the available observations.

Before concluding we wish to mention an interesting aspect of the latter problem. Although the process of collecting observational data takes time, compared with the cosmic scale, the intervals of time involved are negligible and no numerical error is to be expected from the assumption that all the observations thus far accumulated were made simultaneously. However, it must be realized that the observations made on a single photographic plate, recording images of several galaxies, refer to happenings at those galaxies which occurred at widely different epochs in time. The reason is, of course, that, generally, no two galaxies are at the same distances from us, that the variation in these distances runs into millions of light years and that, therefore, the observation of one galaxy A may refer to what happened say 100 million years ago and the simultaneous observation of another galaxy B to the happenings some 200 million years earlier.

Because of this fact, the realization of the hypothetical four-dimensional stochastic process representing the universe is not directly observable. All the observations now available and also those that may be available in the future refer, essentially, to another process, with only two dimensions (symbolized by the angular coordinates of the galaxies), which represents sort of a reflection or an abbreviated projection of the basic four-dimensional process. Here the word *abbreviation* is used to refer to the fact that, owing to great distances, some of the far-away galaxies are too faint to be observed by our instruments. To our minds, the possibility and the intricacies of the study of the basic four-dimensional process, using only its "abbreviated" two-dimensional projection is "a delight and a challenge" to the mathematical statistician and astronomer alike!

In addition, the indeterministic study of cosmology has an aspect for which the word *utilitarian* may not be altogether inappropriate: it so happens that certain questions raised by the deterministic theory, namely the distinction between the various theories of expansion of the universe, cannot be solved through the deterministic approach yet promise a solution through a statistical study (*10, 11*). The point is that the nature of the two-dimensional stochastic process referring to observations—namely the properties of the distribution of images of galaxies on the photographic plates—depends upon whether or not the universe is expanding (strictly speaking, on whether the basic four-dimensional stochastic process is stationary in all four dimensions, including time) and this may be tested by comparisons with observations.

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Radiocarbon from Nuclear Tests

During the past 4 years man has been producing carbon-14 about 15 times faster than nature.

Wallace S. Broecker and Alan Walton

As a result of the generation of neutrons in nuclear tests and their subsequent reaction with nitrogen in the atmosphere, considerable "man-made" C^{14} has been added to the earth's dynamic carbon reservoir. On the basis of the measured neutron yield per unit energy release and on the assumption that about two-thirds of the C^{14} produced falls back as calcium carbonate, Libby (*1*) has estimated that 10×10^{27} atoms of C^{14} have been introduced into the atmosphere as CO_2. Rafter and Fergusson (*2*) have shown that the C^{14} concentration in Southern Hemisphere tropospheric CO_2 has been increasing 2.1 percent per year since 1955. For the Northern Hemisphere, de Vries (*3*) has suggested an increase of 4.3 percent between 1953 and 1957. The results of Munnich and Vogel (*4*) point to an increase of about 3.2 percent per year for central Europe and 2 percent per year in South Africa over the period from 1954 to 1957. Considerable interest has been aroused in the possible genetic effects of this increase in the C^{14} concentration, the potential hazards having been reviewed by Leipunsky (*5*), Pauling (*6*), Sakharov (*7*), and Totter *et al.* (*8*). Recent data related to this increase are presented in this article together with a brief discussion of the probable distribution of bomb C^{14} within the dynamic carbon reservoir.

Results

The general rise in the concentration of C^{14} in atmospheric CO_2 has been monitored by direct measurement of the C^{14}/C^{12} ratio in samples of atmospheric CO_2 as well as by measurement of plant material which has recently fixed atmospheric CO_2. In order to make these measurements directly comparable, all the results are normalized to a common C^{13}/C^{12} ratio. This eliminates any differences created by isotopic fractionation during the photosynthetic process, during the collection of atmospheric CO_2, or during the chemical processing of samples in the laboratory.

The results are summarized in Table 1; δC^{14} represents the age-corrected permillage difference in the radioactivity of the sample CO_2 gas from that of a standard.

$$\delta C^{14} = \frac{A^*_{\text{sample}} - 0.950A^0_{\text{oxalic standard}}}{0.950A^0_{\text{oxalic standard}}} \times 1000 \quad (1)$$

where A^*_{sample} is the measured C^{14} activity of the sample corrected for radioactive decay during the period between sample formation and measurement, and $A^0_{\text{oxalic standard}}$ is the C^{14} activity of the National Bureau of Standards C^{14} standard, corrected for radioactive decay between 1 January 1958 and the measurement date. The factor 0.950 is introduced so that the age-corrected values of wood grown during the 19th century fall close to zero on the scale. Activity measurements were made by the technique described by Broecker *et al.* (*9*); errors are based on the reproducibility of the counting measurements rather than on radioactivity statistics alone.

Following Craig (*10*), the δC^{13} results are expressed as per-millage difference from the Belemnite standard. The isotope ratio measurements were made with a Consolidated 401 double-collecting mass-spectrometer on the CO_2 gas used for the radioactivity measurement (*11*). The uncertainty in the δC^{13} results is about 1 per mill.

Normalization to a common C^{13}/C^{12} ratio is carried out as follows:

$$\Delta C^{14} = \delta C^{14} - 2\delta C^{13}\left[1 + (\delta C^{14}/1000)\right] - 50.0 \quad (2)$$

Again, the constant term (−50.0) is introduced so that ΔC^{14} for age-corrected 19th-century wood falls close to zero on the scale (*12*).

The results from the Northern Hemisphere are plotted against time in Fig. 1 and suggest a general increase of about 50 per mill (5 percent) per year in the C^{14} concentration of atmospheric CO_2 over the past 3 years. This is somewhat higher than the estimates given by de Vries (*3*) and Munnich and Vogel (*4*). The spread of the points in Fig. 1 must be related at least in part to local release of large quantities of industrial CO_2 (*13*). Consequently, the curve has been drawn through points obtained on samples collected in areas removed from intense industrial activity (the Great Basin, the North Atlantic Ocean, and the Mediterranean Sea). The horizontal portion of this curve has been fixed by using Fergusson's value of −20 per mill for the average Suess effect (*13*). It is interesting to note that data obtained by Munnich and Vogel (*4*) on plant material from Germany show, in general, a smaller increase than the data that define the curve in Fig. 1. If plotted in Fig. 1, these data would agree well with the data from Kearney, Palisades, and Rome.

The difference between the C^{14} concentration in leaf samples grown at Lamont Geological Observatory and that in average Northern Hemisphere tropospheric CO_2 appears to be increasing with time. Such an increase would not be expected if the difference were a result of a higher local Suess effect. The suggestion of Munnich and Vogel (*4*) that plants grown in areas of dense vegetation may take up an appreciable quantity of CO_2 given off by adjacent soils provides a possible explanation for such a divergence. For, since much of the CO_2 given off by soils probably results from the decay of organic materials formed prior to bomb testing, the contrast between the C^{14} concentration in soil CO_2 and that in atmospheric CO_2 is increasing rapidly with time. In this connection it is of interest to note that the points defining the curve in Fig. 1 are all from areas of sparse

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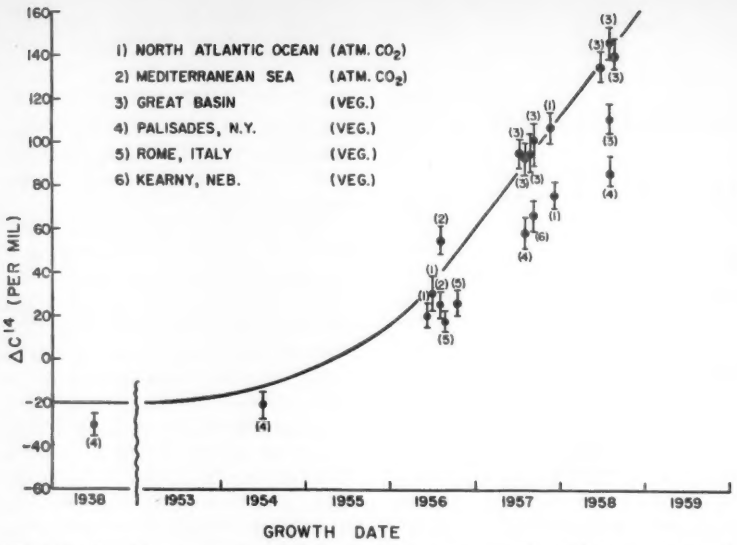
1) NORTH ATLANTIC OCEAN (ATM. CO_2)
2) MEDITERRANEAN SEA (ATM. CO_2)
3) GREAT BASIN (VEG.)
4) PALISADES, N.Y. (VEG.)
5) ROME, ITALY (VEG.)
6) KEARNY, NEB. (VEG.)
ΔC^{14} (PER MIL)
GROWTH DATE

Fig. 1. Carbon-14 concentration of atmospheric CO_2 in the Northern Hemisphere plotted against time.

vegetation and thus are free from influence of soil CO_2 as well as from that of local industrial CO_2.

Figure 2 compares the data for the Northern Hemisphere (Fig. 1) with those published by Rafter and Fergusson (*2*) for the Southern Hemisphere. In drawing the curve for the Southern Hemisphere it was assumed that the Lamont and New Zealand 1890 wood standards have identical activities. Recent measurement of the New Zealand wood standard at Lamont supports this assumption (the New Zealand laboratory pre-1900 wood average is within 10 per mill of the value for the Lamont 1890 wood standard). The two added points represent Lamont data from the Southern Hemisphere. In the following paragraphs it is shown that the proximity of the two curves lends support to Fergusson's (*13*) conclusion (based on the distribution of industrial CO_2) that the mixing rate between the two hemispheres must be quite rapid (less than 2 years).

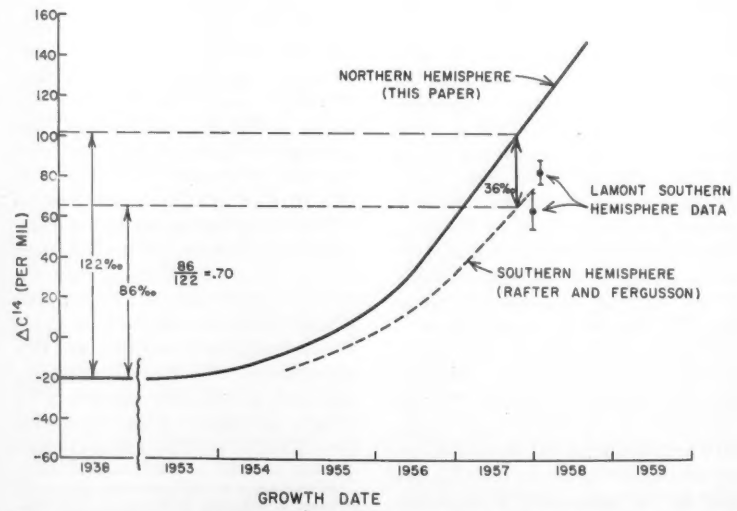

Fig. 2. Comparison of atmospheric C^{14} data for the Northern Hemisphere with data published by Rafter and Fergusson (*2*) for the Southern Hemisphere.

Interhemispheric Mixing

Since most of the bomb-produced C^{14} (bomb C^{14}) is added to the stratosphere of the Northern Hemisphere, there are two possible modes of mixing between the hemispheres: mixing above the tropopause and below the tropopause. Two extreme cases may then be considered: (i) transfer of bomb C^{14} from the Northern Hemisphere to the Southern Hemisphere dominantly in the stratosphere, and (ii) transfer dominantly in the troposphere. In the absence of a more precise model for atmospheric mixing, the simple model of four internally mixed reservoirs (Northern and Southern Hemisphere tropospheres and stratospheres) separated by semipermeable membranes is used.

In the first case, the rate of change in the number of bomb-produced C^{14} atoms in the Southern Hemisphere troposphere (N_{S_T}) at any time t is given by:

$$\frac{dN_{S_T}}{dt} = \lambda_1 N_{S_S} \qquad (3)$$

where λ_1 is the mixing coefficient between the stratosphere and troposphere and N_{S_S} is the number of bomb-produced C^{14} atoms in the Southern Hemisphere stratosphere. In words, this equation simply says that the rate at which the amount of tropospheric bomb C^{14} increases is directly proportional to the amount of bomb C^{14} in the stratospheric source. The troposphere is so much more massive than the stratosphere that the return flux of C^{14} to the stratosphere is neglected in Eq. 3. In the case of intrastratospheric transfer, however, both directions are important, so the equation defining the rate of increase in the Southern Hemisphere stratosphere is as follows:

$$\frac{dN_{S_S}}{dt} = N_{N_S}\lambda_2 - N_{S_S}\lambda_2 \qquad (4)$$

where λ_2 is the stratosphere mixing coefficient between the Northern and Southern Hemispheres and N_{N_S} is the number of bomb-produced C^{14} atoms at any given time in the Northern Hemisphere stratosphere. An equation similar to Eq. 3 can be written for the stratosphere-to-troposphere transfer in the Northern Hemisphere:

$$\frac{dN_{N_T}}{dt} = \lambda_1 N_{N_S} \qquad (5)$$

Since the rate of increase, dN_{N_T}/dt, is proportional to the slope of the curve

in Fig. 2, and since this slope is approximately constant after early 1955, dN_{NT}/dt is set equal to a constant, B. Again, the return flux, as well as any loss of bomb C^{14} to the ocean or biosphere, is neglected (see below).

Simultaneous solution of the above equations yields

$$N_{ST} = Bt - \frac{B}{\lambda_2}(1 - e^{-\lambda_2 t}) \qquad (6)$$

It is assumed that at time zero (March 1955) all the N terms are zero. Since $N_{NT} = Bt$, it follows that

$$\frac{N_{ST}}{N_{NT}} = 1 - \frac{1}{\lambda_2 t}(1 - e^{-\lambda_2 t}) \qquad (7)$$

From Fig. 2 it may be seen that the tropospheric bomb C^{14} concentration in the Southern Hemisphere was 0.70 times that in the Northern Hemisphere in September 1957—that is, at $t = 2.5$ years, $N_{ST}/N_{NT} = 0.70$. Substitution of these values in Eq. 7 shows λ_2 to be about 1.3. Since the reciprocal of λ_2 is mean residence time, it follows that the average bomb-produced C^{14} atom spends about 1 year in the Northern Hemisphere stratosphere before it enters the Southern Hemisphere stratosphere.

In the second extreme case, bomb C^{14} travels to the Southern Hemisphere solely through the troposphere. The equation defining the rate of increase of Southern Hemisphere tropospheric bomb C^{14} has the form of Eq. 4, as follows:

$$\frac{dN_{ST}}{dt} = N_{NT}\lambda_3 - N_{ST}\lambda_3 \qquad (8)$$

When combined with the previously used relationship, $N_{NT} = Bt$, and integrated, Eq. 8 becomes identical to Eq. 6, except that λ_3 replaces λ_2. Similarly, Eq. 7 will represent case 2 if λ_3 replaces λ_2. This means that the reciprocal of λ_3, the mean residence time of a bomb-produced C^{14} atom in the Northern Hemisphere troposphere, is also about 1 year. Although somewhat lower, this value is consistent with that of 1.5 years based on a similar calculation by Munnich and Vogel (*4*).

The model on which these results are based is admittedly greatly oversimplified, for, among other things, no attempt was made to take into account direct addition of bomb C^{14} to the Southern Hemisphere atmosphere during tests carried out near the equator or in the Southern Hemisphere itself. Consequently, it can only be said that mixing between the hemispheres is rapid enough so that either the entire stratosphere or the entire troposphere mixes horizontally with a mean mixing time of less than 2 years. From the bomb C^{14} data alone it is impossible to determine whether the rapid mixing occurs above, below, or possibly both above and below the tropopause.

Quantity and Distribution of Bomb-Produced Carbon-14

Two major questions next arise: (i) What is the total quantity of bomb C^{14} added to the dynamic carbon cycle up to March 1958? and (ii) If no more bomb C^{14} were added after March 1958, how would the bomb C^{14} concentration in the atmosphere change with time?

The answer to the first question requires not only a knowledge of the

Table 1. Data showing the rise in the C^{14} concentration of atmospheric CO_2 as a result of nuclear testing.

Growth date	Location	Material	δC^{14}	δC^{13}	ΔC^{14}	Lamont No.
1875	Pacific Northwest (U.S.)	Spruce wood	0 ± 6	− 21.4	− 7 ± 6	L-353D
1890	Palisades, N.Y.	Oak wood	1 ± 5	− 24.0	0 + 5	L-314
1938	Palisades, N.Y.	Oak wood	− 29 ± 5	− 24.1	− 32 ± 5	L-313
1954	Palisades, N.Y.	Oak leaves	− 17 ± 6	− 23.2	− 21 ± 6	L-316B
Pre-1955	Lake Winnemucca, Nev.	Sage wood	4 ± 7	− 11.2	− 23 ± 7	L-288M
June 1956	North Atlantic (34°N, 64°W)	Atmospheric CO_2	62 ± 8	− 9.0	31 ± 8	L-367A
June 1956	North Atlantic (32°N, 59°W)	Atmospheric CO_2	54 ± 6	(− 8 ± 4)	(21 ± 7)	L-367B
July 1956	Mediterranean Sea (41°N, 13°E)	Atmospheric CO_2	88 ± 8	− 7.4	54 ± 8	L-367C
July 1956	Mediterranean Sea (36°N, 23°E)	Atmospheric CO_2	64 ± 6	− 5.3	25 ± 6	L-367D
Nov. 1956	Rome, Italy	Poplar twigs	17 ± 5	− 25.0	18 ± 5	L-371B
Nov. 1956	Rome, Italy	Grain	23 ± 6	− 25.1	24 ± 6	L-371A
Oct. 1957	Palisades, N.Y.	Leaves	53 ± 7	− 25.6	57 ± 7	L-445
Aug. 1957	Kearney, Neb.	Leaves	48 ± 7	− 31.8	65 ± 7	L-415B
Aug. 1957	Evanston, Wyo.	Leaves	83 ± 7	− 28.2	94 ± 7	L-415G
Aug. 1957	Oquirrh Mtn., Utah	Leaves	89 ± 7	− 26.0	96 ± 7	L-415EEE
Aug. 1957	Wadsworth, Nev.	Leaves	85 ± 7	− 27.8	95 ± 7	L-415HH
Aug. 1957	Truckee, Calif.	Leaves	91 ± 7	− 27.2	100 ± 7	L-415QQ
Nov. 1957	North Atlantic (25°N, 56°W)	Atmospheric CO_2	131 ± 7	− 10.8	105 ± 7	L-464C
Dec. 1957	North Atlantic (11°N, 35°W)	Atmospheric CO_2	109 ± 6	− 7.2	75 ± 6	L-466B
Jan. 1958	South Atlantic (33°S, 50°W)	Atmospheric CO_2	94 ± 9	− 8.8	63 ± 9	L-466C
Feb. 1958	South Atlantic (54°S, 64°W)	Atmospheric CO_2	103 ± 6	− 13.1	82 ± 6	L-466D
Oct. 1958	Palisades, N.Y.	Leaves	77 ± 5	− 27.0	85 ± 5	L-487S
Sept. 1958	Evanston, Wyo.	Leaves	142 ± 7	− 23.7	146 ± 7	L-437B
Sept. 1958	Oquirrh Mtn., Utah	Leaves	111 ± 7	− 22.6	111 ± 7	L-487C
Aug. 1958	Wadsworth, Nev.	Leaves	131 ± 9	− 26.1	140 ± 9	L-487D
Aug. 1958	Truckee, Calif.	Leaves	126 ± 7	− 26.1	135 ± 7	L-487E

Table 2. Summary of C^{14} measurements on dissolved bicarbonate from the North Atlantic and equatorial Atlantic surface waters.

Collection date	No. of samples	Average ΔC^{14} (0/00)	Standard deviation from mean (0/00)	Standard error of mean (0/00)
		North Atlantic		
Nov. 1955	8	−49	± 8	± 3
June 1956	4	−48	± 9	± 5
June 1957	1	−44 ± 7		
Oct. 1957	1	−23 ± 7		
Nov. 1957	4	−33	± 12	± 6
		Equatorial Atlantic		
Feb. 1956	2	−61	± 7	± 5
Jan. 1957	2	−62	± 13	± 9
May 1957	1	−68 ± 7		
Dec. 1957	1	−48 ± 9		

amount of bomb C^{14} in the troposphere but also of the amounts which have entered the ocean and the terrestrial biosphere as well as of the amount still stored in the stratosphere.

If complete mixing within each hemisphere is assumed, the amount of bomb C^{14} in the troposphere can be estimated directly from Fig. 2 and a knowledge of the pre-bomb tropospheric C^{14} inventory (25×10^{27} atoms). Up to March 1958 the amounts are 1.5×10^{27} atoms of bomb C^{14} in the Southern Hemisphere troposphere and 2.1×10^{27} atoms for the Northern Hemisphere.

The amount of bomb C^{14} which has entered the terrestrial biosphere can be estimated in the following manner. The rate of change of bomb C^{14} in the biosphere may be written as

$$\frac{dN^*_B}{dt} = \frac{R_P N^*_T}{N_T} - N^*_B \lambda_{B-T} \qquad (9)$$

where N^*_T and N^*_B are the number of bomb-produced C^{14} atoms in the troposphere and biosphere at any given time t; N_T is the number of stable carbon atoms in the troposphere; R_P is the rate of photosynthesis by terrestrial plants (that is, atoms of carbon fixed per unit time); and λ_{B-T} is the average rate of decay of terrestrial plant material back to CO_2 (that is, atoms of carbon decaying per unit time per atom of biospheric carbon). When N^*_T is expressed as $B't$, where B' is the observed constant rate of increase in the number of atoms of bomb C^{14} in the total troposphere (Northern plus Southern Hemispheres), the following relationship results from integration of Eq. 9:

$$N^*_B = \frac{R_P B'}{N_T \lambda_{B-T}} \left[t - \frac{1}{\lambda_{B-T}} (1 - e^{-t\lambda_{B-T}}) \right] \qquad (10)$$

As before, the rate of increase, B', is computed on the assumption that the bomb C^{14} concentration in the troposphere has increased at a linear rate since March 1955—an approximation close enough to the actual situation so that no significant error is introduced. From data in Fig. 2, $B' = 1.2 \times 10^{27}$ atom/yr. From Goldschmidt (*14*), R_P is estimated to be about 20×10^{15} grams of carbon per year, and $1/\lambda_{B-T}$, the mean life of carbon in the biosphere, to be about 15 years. When these values are substituted in Eq. 10, the value of N^*_B turns out to be 0.2×10^{27} atoms. Since this is small compared with the bomb carbon entering other reservoirs, the relatively large uncertainties associated with R_P and λ_{B-T} are not critical to the over-all inventory.

The amount of bomb C^{14} which has entered the ocean can be estimated from Craig's (*15*) value of mean residence time of CO_2 in the atmosphere with respect to entry into the ocean. If the rate, A, for addition of bomb C^{14} to the troposphere, is considered to be constant (this was essentially the case over the 3-year period from March 1955 to March 1958), and if loss of bomb C^{14} to the biosphere is neglected, then the number of bomb-produced C^{14} atoms which have entered the ocean, N^*_O, up to time t is simply the difference between the total amount added to the troposphere, At, and the amount, N^*_T, in the troposphere at time t; that is

$$N^*_O = At - N^*_T \qquad (11)$$

When differentiated with respect to t, Eq. 11 becomes

$$dN^*_O/dt = A - (dN^*_T/dt) \qquad (12)$$

The rate at which bomb C^{14} enters the ocean is assumed to be directly proportional to the amount of bomb C^{14} present in the troposphere, and the return flux from ocean to atmosphere is considered negligible; therefore

$$dN^*_O/dt = \lambda_{T-O} N^*_T \qquad (13)$$

where λ_{T-O} is the mixing coefficient for transfer of CO_2 from troposphere to ocean. Combining Eqs. 12 and 13 to eliminate N^*_O and integrating the result gives

$$N^*_T = \frac{A}{\lambda_{T-O}} (1 - e^{-\lambda_{T-O} t}) \qquad (14)$$

Substituting this result into Eq. 11 gives

$$N^*_O = A \left[t - \frac{1}{\lambda_{T-O}} (1 - e^{-\lambda_{T-O} t}) \right] \qquad (15)$$

By using the limits of 0.25 and 0.10 yr^{-1} given by Craig (*15*) for λ_{T-O} and a value of A based on B' and an iterative correction for loss to the ocean, upper and lower limits of 1.5×10^{27} and 0.6×10^{27} atoms of C^{14} are obtained for N^*_O, with a most probable value of 1.0×10^{27} atoms. This indicates that only about one-fourth of the bomb C^{14} added to the troposphere has entered the ocean.

If the bomb C^{14} that has entered the ocean were concentrated entirely in the upper 100 meters, the C^{14}/C^{12} ratio in average surface ocean bicarbonate should be from 12 to 32 per mill higher than in 1955. Table 2 summarizes actual measurements on samples from the surface of the north and equatorial Atlantic over the two-year period from 1955 to 1957. Although the results suggest a small increase (~ 15 per mill), the uncertainties are large enough so that no precise estimate of the magnitude of the

Table 3. Inventory of bomb-produced C^{14} as of March 1958 (10^{27} C^{14} atoms).

	Troposphere N^*_T	Terrestrial biosphere N^*_B	Ocean N^*_O	Stratosphere N^*_S	Total $N^*_T + N^*_B + N^*_O + N^*_S$
Minimum	3.6	0.2	0.6	0.7	5.1
Most probable	3.6	0.2	1.0	7.0	11.8
Maximum	3.6	0.2	1.5	22.5	27.8

increase can be given. It appears, however, to be less than 30 per mill and is thus consistent with the calculated estimate. It should be noted that these calculations involve only the bomb C^{14} which is converted into CO_2 and not that which falls into the ocean as $CaCO_3$. The fate of the latter is not known.

From these data the fraction of bomb C^{14} which has entered the ocean appears to be somewhat lower than that given by Munnich and Vogel (*4*), who based their estimate on the atmospheric CO_2 residence time given by Rafter and Fergusson (*2*) (1.4 years) rather than on the value of Craig (*13*) (7 years) used in this study. The Rafter and Fergusson value is based on the 18 per mill increase in C^{14} concentration of the dissolved bicarbonate in surface ocean water from Markara Bay, off New Zealand, over the period from November 1954 to May 1957. Both the later measurements by Rafter and Fergusson (*2*), which indicate no further rise over the period from May 1957 to March 1958, and the data on the Atlantic Ocean presented in this article suggest that the 1.4-year value is not a reliable estimate.

The critical factor in the over-all inventory is the amount of bomb C^{14} stored in the stratosphere. As pointed out by Libby (*1*), most H-bomb clouds rise well into the stratosphere, and so it is reasonable to assume that most of the newly produced bomb C^{14} is added to the stratosphere. In this case a minimum estimate of the amount stored in the stratosphere would certainly be obtained if the fractional increase in stratospheric C^{14} were assumed to be the same as that measured for the troposphere. Since the stratosphere contains only about 20 percent of the amount of CO_2 in the troposphere, the minimum is thus 0.7×10^{27} atoms of C^{14}.

A maximum estimate can be made by taking the highest estimates of stratospheric residence time calculated for fission-product debris and by assuming that all bomb C^{14} added to the lower atmosphere comes down from the stratosphere. In this case

$$N^*_S = A/\lambda_{S-T} \qquad (16)$$

where N^*_S is the number of bomb C^{14} atoms in the stratosphere, A is the rate of addition of C^{14} to the troposphere from the stratosphere, and λ_{S-T} is the transfer coefficient from stratosphere to troposphere. Again, return flow can be neglected since the maximum residence time estimate is appreciably longer than the period of large-scale bomb testing. If the value of A (1.5×10^{27} atom/yr) computed in connection with Eq. 15 is used and 15 years is taken as an upper limit for the mean stratospheric storage time (*16*), a value of 22.5×10^{27} atoms is obtained as an upper limit on N^*_S. If this is actually the case, the amount of bomb C^{14} in the average mole of stratospheric CO_2 must be about 30 times greater than that in each mole of tropospheric CO_2. If the value of 5 years given by Machta (*16*) is taken as the best estimate, a value of 7×10^{27} atoms is obtained.

When the upper and lower limits for each reservoir are summed, limits can be set on the total amount of bomb C^{14} that had been added to the dynamic carbon reservoir through March 1958. The results, as summarized in Table 3, suggest an upper limit of 28×10^{27} and a lower limit of 5×10^{27} atoms (best estimate, 12×10^{27} atoms). These values may be compared with the estimate of 10×10^{27} atoms given by Libby (*1*) for the same period. The latter estimate is based on the neutron flux per megaton and the number of megatons exploded, rather than on direct measurements of C^{14} concentration. Since there are at present about 2000×10^{27} cosmic-ray-produced C^{14} atoms in the dynamic reservoir, the bomb C^{14} represents a total increase of from 0.25 to 1.4 percent (best estimate, 0.6 percent). Thus, complete mixing will result in a 50-fold reduction in the atmospheric bomb C^{14} concentration if the lower estimate is used and a 20-fold reduction if the upper value is used (provided there is no further production of C^{14} by bombs).

An important question is: How long will the mixing process require? Since great uncertainties exist concerning (i) the rate of exchange between the stratosphere and troposphere, (ii) the size and turnover rate of the biosphere, and (iii) the patterns and rates of mixing in the deep ocean, precise estimates must await further work. A contribution to (iii) is the extensive program currently being carried out at Lamont to determine mixing rates in the Atlantic Ocean (*17*).

Conclusions

The following conclusions can be drawn from the present study (*18*): (i) The C^{14} concentration in the tropospheric CO_2 of the Northern Hemisphere has increased by about 5 percent per year between March 1955 and March 1958. (ii) The distribution of bomb C^{14} between the hemispheres suggests that horizontal mixing must be so rapid that either the stratosphere or the troposphere becomes mixed in less than two years. (iii) Only about 10 percent of the bomb C^{14} produced up to March 1958 has entered the oceans. (iv) Differences within one hemisphere appear to be related to local dilution with C^{14}-deficient CO_2. (v) The total bomb C^{14} added to the dynamic carbon reservoir through March 1958 is between 5×10^{27} atoms and 28×10^{27} atoms. The range results largely from uncertainty about the amount stored in the stratosphere. (vi) When the bomb C^{14} produced through March 1958 is distributed uniformly throughout the dynamic reservoir, the concentration of C^{14} in the atmosphere will be 0.25 to 1.41 percent higher than in the pre-bomb era. This increment will then decay away, with a half-life of 5600 years. (vii) Critical factors still to be determined are the residence time of gas molecules in the stratosphere and the rate of mixing between surface waters and the deep ocean (*19*).

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18. This research was carried out at the Lamont Geological Observatory of Columbia University. Financial support for the oceanic sampling and analyses program was provided by the Atomic Energy Commission [grant AT (30-1) 1808] and by the International Geophysical Year Organization (grant Y/9.11/134). The terrestrial samples were measured in connection with a program supported by the National Science Foundation (grant N.S.F. G 4191). We appreciate the help and encouragement provided by J. L. Kulp and the extremely valuable review and criticism of the manuscript by E. A. Olson.
19. This paper is Lamont Geological Observatory contribution No. 356.

Emotion in Prejudice

Physiological tests support the thesis that prejudicial attitudes are attended by relatively strong emotion.

Joseph B. Cooper

Attitudes are viewed psychologically as learned sets, preparations for action toward particular stimulus objects. Although some attitudes are referred to as prejudicial, the specific characteristics which social psychologists agree upon as essential criteria for judging whether or not an attitude is prejudical are not fully understood.

One of the characteristics most investigators agree upon as an essential criterion for designating an attitude as prejudicial is level of emotionality. In fact some psychologists (*1*) have gone so far as to define prejudice as ". . . an emotional attitude." Gordon Allport (*2*) has stated this thesis operationally in the following way: "We tend to become emotional when a prejudice is threatened with contradiction."

Grounds for the thesis that prejudicial attitudes are supported by emotion are not especially convincing when viewed empirically. It may be that the thesis is acceptable to many psychologists partly because there seems to be no contradictory evidence. What supporting evidence there is has come principally from two sources. First, when a person verbally expresses a strong negative or positive attitude toward some human group, or defends such an attitude in the face of contradiction, he commonly displays behavior which is interpreted by others as emotional. Second, emotionally charged words and phrases used in attitude scales are often selected by subjects as descriptive of their attitudes toward certain human groups (*3*). "Attitude scale" refers to any paper-and-pencil device to which subjects individually respond either by placing a given stimulus group (for example, ethnic or national) at some position on a preference continuum, or by checking a statement which is descriptive of the individual subject's feeling toward a given stimulus group.

If it is true that prejudicial attitudes are supported by emotion, it should be possible to detect the presence of emotion by physiological means as well as by attitude scale content analysis and overt behavior observation. Attitude scale "content analysis" is a procedure whereby statements which are descriptive of many possible feelings toward stimulus groups are categorized according to psychological meaning, that is, such meaning categories as emotional strength, type of emotion, cognitive state, and so forth. By "overt behavior observation" is meant observing humans react, and recording their reactions in accordance with certain categories; for example, emotional and nonemotional responses are tabulated. To our knowledge, physiological measurement has not been used to test the thesis that prejudicial attitudes which are subjected to verbal contradiction are supported by relatively high levels of emotionality (*4*).

It is known that emotion involves widespread physiological changes which have the effect of mobilizing the body to meet emergency. The galvanic skin response is regarded as a reasonably valid index of such physiological changes. When this response indicates a decrease in skin resistance, the inference is drawn that physiological mobilization has increased. Upon the basis of such an increase in physiological mobilization, the further inference is drawn that the level of emotion has increased. For a given subject, galvanic skin responses will provide an ordinal index of emotional level. That is, one measurement may be said to be greatest, another next greatest, and so on to that which is least. However, the response affords no information with respect to the cognitive direction an emotion may take. That is to say, the skin response does not distinguish scorn from fear, rage from joy.

This article (*5*) describes three exploratory studies, each designed to test the validity of the thesis that prejudicial attitudes are supported by relatively high levels of emotionality.

Responses to Verbal Contradiction of Positive and Negative Attitudes

In the first study (*6*) college students were asked to rate and rank 20 alphabetically listed ethnic and national groups in terms of preference. Subjects rated each group independently by checking one of six graded preference categories: "like intensely" to "dislike intensely." They ranked the groups by designating the group liked best, next best, and so on, to the group liked least. The groups were: Argentines, Austrians, Canadians, Chinese, English, French, Germans, Greeks, Indians (India), Irish, Italians, Japanese, Jews, Mexicans, Negroes, Filipinos, Poles, Russians, Swedes, Turks. Interest did not reside in the particular named groups, but rather in each subject's negative and positive attitude-indicating responses to groups as groups.

Of 126 subjects, 26 confirmed the validity of their highest and lowest rankings

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(positions 1 and 20) by also rating those groups high and low, respectively. It was assumed that extreme rankings which were confirmed by extreme ratings identified prejudicial attitudes. Twenty of these 26 subjects were subsequently available for individual laboratory sessions.

The equipment used to measure galvanic skin responses was a Stoelting psychogalvanoscope. With this equipment, resistance changes were indicated by a dial needle which reflected "units of resistance." Deflection magnitudes could not be directly compared from subject to subject since subjects balanced in at different response (resistance) levels. However, for a given subject in a given session, magnitudes of needle deflections could be directly compared. Thus, for a given subject, it was possible to obtain both ordinal and interval values of several possible emotion-evoking stimuli. That is, for a given subject, not only could his galvanic skin response magnitudes be ordered to ranks, but they could be assigned quantitative values which permitted their conversion to ratios. Because needle excursions varied not only in space covered but also in duration, the responses were estimated as the product of needle deflection multiplied by time. This was the procedure followed in this study. During the presentation of a stimulus by the experimenter the equipment operator observed two things, needle deflection and time. If and when the needle passed the fifth calibration mark (the pin was at 35 maximum) he started his stop watch. He stopped his watch when the needle came to the fifth calibration mark on its return.

Four brief evaluation statements were composed for use in the individual experimental sessions. Each was designed so that the name of any group could be inserted into it. Two of the statements could be used to place any inserted group name in a derogatory light, and the other two to place any inserted group name in a complimentary light. For a given subject, the name of his most liked group was inserted into one of the derogatory statements, and the name of his most disliked group was inserted into one of the complimentary statements. The names of the groups that the subject ranked in positions 10 and 11 were inserted into the two remaining statements—one derogatory and one complimentary. The order of presentation of the statements and the insertion of ranked (as confirmed by ratings) group names was randomized from subject to subject. The equipment operator had no way of knowing either the content or strength of the subject's scaled attitude toward any group.

One of the derogatory statements was: "People can be divided into two groups: the good and the bad. Close to the bottom of the list are the ———. They certainly can be said to have caused more trouble for humanity than they are worth." One of the complimentary statements was: "The world over, no single group of people has done as much for us, for our civilization, as the ———. The world will undoubtedly come to recognize them as honest, wise and completely unselfish." The other statements were similar to these.

After a subject had been balanced in, a trial statement which incorporated the name of the group the subject had ranked in position 9 was read. Following this the four critical statements were read.

Since galvanic skin response raw scores could not be directly compared from subject to subject, each subject's laboratory session was treated as an independent experiment. That is, intrasubject rather than intersubject comparisons were made. Thus, 20 separate experiments were conducted. The problem was reduced to determining in how many experiments the critical stimuli evoked relatively greater responses than did the neutral stimuli. The data were analyzed in two ways.

In the first analysis, each subject's skin responses to the names of the groups he had ranked in positions 10 and 11 were averaged. It was then determined by simple inspection whether or not responses evoked by critical stimuli (those relating to most liked and most disliked groups) were larger or smaller than the mean of the responses to the noncritical stimuli. These data are shown in Table 1, "First Study." It will be noted that for 14 of the subjects, skin responses were greater to derogatory statements containing the names of liked groups than to statements containing the names of groups toward which they had expressed a relative attitude of neutrality. The binomial test indicated a probability of .059. This provided minimal evidence for the thesis that positive (that is, favorable) prejudicial attitudes are supported by relatively great emotionality. However, for 19 of the 20 subjects, galvanic skin responses were greater to complimentary statements containing the names of disliked groups than to those containing the names of neutrals. In this instance $p < .001$. Thus, substantial support for the thesis that negative (that is, unfavorable) prejudicial attitudes are

Table 1. Galvanic skin responses, in resistance units, to verbal stimuli. Stimuli under "No. 1" consisted of derogatory statements about the group most liked by the subject. Stimuli under "No. 20" consisted of complimentary statements about the group most disliked by the subject. Greater refinement in measurement in the second study accounts for the decimals. The subjects in the first study were not the same as those in the second.

	First study			Second study	
Subject	Mean of Nos. 10 and 11	No. 1	No. 20	Mean of Nos. 10 and 11	No. 20
1	170	8	247	11.30	47.14
2	83	120	88	10.75	16.45
3	593	1400	700	59.60	69.30
4	317	1575	360	8.95	1.20
5	5	600	550	77.92	206.10
6	952	992	2100	11.60	46.00
7	169	70	84	29.29	28.30
8	187	420	525	18.00	39.20
9	30	25	48	6.76	24.64
10	96	20	325	18.32	25.79
11	29	160	91	6.69	18.60
12	145	88	150	32.24	72.90
13	49	105	310	5.20	8.29
14	42	70	60	29.30	23.30
15	346	364	350	10.12	36.72
16	161	308	432	10.25	27.35
17	15	35	208	23.74	41.13
18	421	520	594	24.81	60.62
19	42	12	200	57.68	113.51
20	49	208	180	18.97	150.50
21				5.17	23.70
22				14.16	22.95
23				44.79	53.61

supported by relatively great emotionality was provided by this result.

The second analysis was purely descriptive. Ratios for each subject's skin responses were computed for the following: neutral attitude response (rank position 10) to neutral attitude response (rank position 11), positive attitude response to mean neutral attitude response, and negative attitude response to mean neutral attitude response. When these three distributions of ratios (Table 2, "First study," and Fig. 1) were compared, one significant observation emerged. The ratios of neutral to neutral stimulus-evoked responses clustered around unity; the variability was relatively slight. For both the negative attitude and positive attitude stimulus-evoked responses to neutral stimulus-evoked responses, the ratio variability was clearly much greater. This lends support to the contention that the group name stimulus was principally responsible for response magnitude, rather than the evaluation statement itself.

Responses to Verbal Contradictions of Negative Attitudes—Partial Replication

The second study (7) was designed as a partial replication of the first. In view of the fact that in the first study only one of 20 subjects failed to respond to a negative attitude stimulus with greater emotionality than to the mean of neutral stimuli, it was apparent that further study of this relationship was warranted. Thus, the second study dealt only with negative prejudicial attitudes.

The same rating and ranking scales that were used in the first study were administered to 176 college students. Of these, 31 gave extremely low ratings to the groups they ranked lowest. Twenty-three of these subjects were subsequently available for skin response sessions.

A self-recording galvanic skin response unit of a Keeler polygraph was used in place of the dial-indicator Stoelting psychogalvanoscope. This polygraph is equipped with a kymograph which carries chart paper at a uniform rate beneath a recording pen. Skin response magnitude may be calculated by measuring the area under the curve between two given points.

Four 18-word complimentary statements were so constructed that the name of a selected group could be inserted as the second word. One of the statements was: "The ——— have demonstrated to the rest of the world that their excellence is justly and honestly earned."

Subjects were individually escorted into the laboratory room by the experimenter. Rapport was established by introducing the subject to the equipment operator, and briefly describing the equipment and the manifest purpose of the experiment. After the subject was comfortably seated in a specially designed chair, directions were read by the experimenter. The directions included a statement instructing the subject to refrain from overtly verbalizing about any of the statements. Subjects were urged to "concentrate on the statement, imagining how you feel about it or how you would respond." This control was introduced with the intention of reducing the possibility that part of the measured affectivity might be a function of preparing, responding, and listening to one's own speech in a social setting. The subject was seated facing the experimenter, slightly to the right of the equipment and operator. His position was such that he could see both the equipment and the operator but could not observe the pen tracings.

The equipment operator then attached palmar electrodes. While the experimenter read the directions, the equipment operator began balancing the subject into the bridge circuit. After balancing in had been accomplished, the operator nodded to the experimenter, who, after a pause of about 10 seconds, began reading the first statement. The first statement was the same for each subject and contained the name of the group the subject had placed in rank preference position 9. This was for the purpose of validating the skin response base line which had been established for the subject. Into the three remaining state-

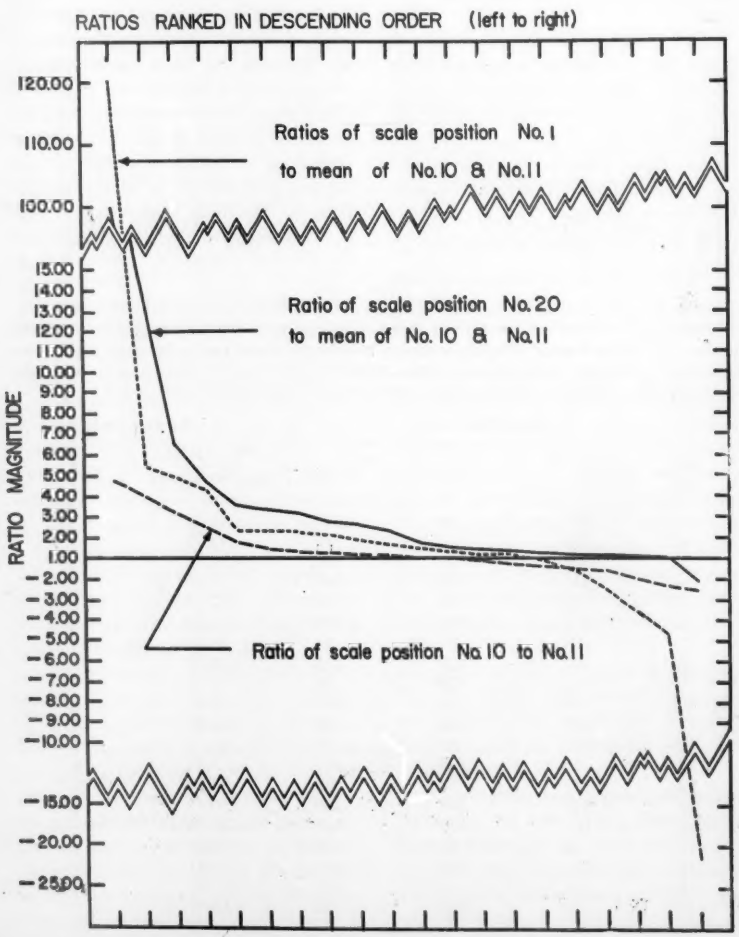

Fig. 1. Ratio distributions from the first study. Ratios of scale positions No. 10 to 11 are relatively restricted, approximately the same number falling above and below unity.

ments were inserted the names of the groups the subject had ranked at positions 10, 11, and 20. The orders of presentations of both the group names and the statements were randomized from subject to subject.

The same data analyses were made in this study as in the first. Each subject's galvanic skin responses to his rank position 10 and 11 stimuli were averaged. This mean magnitude was then compared with the magnitude of his response to the statement containing the name of his 20th rank position. These data are shown in Table 1, "Second study." In 20 of the 23 cases, responses were greater to statements containing the subject's most disliked groups. The binomial test indicated $p < .001$. Thus, this finding confirmed its counterpart in the first study.

Ratios for each subject's galvanic skin responses were computed as follows: response for the statement containing the group name of rank position 20 (disliked) to the mean response for preference positions 10 and 11 (neutrals), and the response for rank position 10 to the response for rank position 11. When these ratios were rank distributed (Table 2, "Second study," and Fig. 2) the same pattern that was found in the first study appeared. The ratios of negative to neutral stimulus-evoked responses were widely distributed, whereas those of neutral to neutral were relatively restricted. This tends to confirm the results of the first study, which led to the inference that the group name stimulus is an independent variable of importance in determining skin response magnitude.

Attitudes Predicted from Skin Response Measurements—Reverse Design

In the third study (*8*) the basic design of the first and second studies was reversed. Subjects were first measured for galvanic skin responses to complimentary statements in reference to nine ethnic and national groups. At a later time these subjects were administered an attitude scale containing the names of the same groups. From skin response records, predictions were made with reference to subjects' scale-measured attitudes. The basic prediction was: an excessive galvanic skin response to a complimentary statement concerning a group identifies that group as the object of a negative prejudicial attitude. In this study, subjects were not first screened and selected upon the basis of attitude scale results. Nothing was known of a

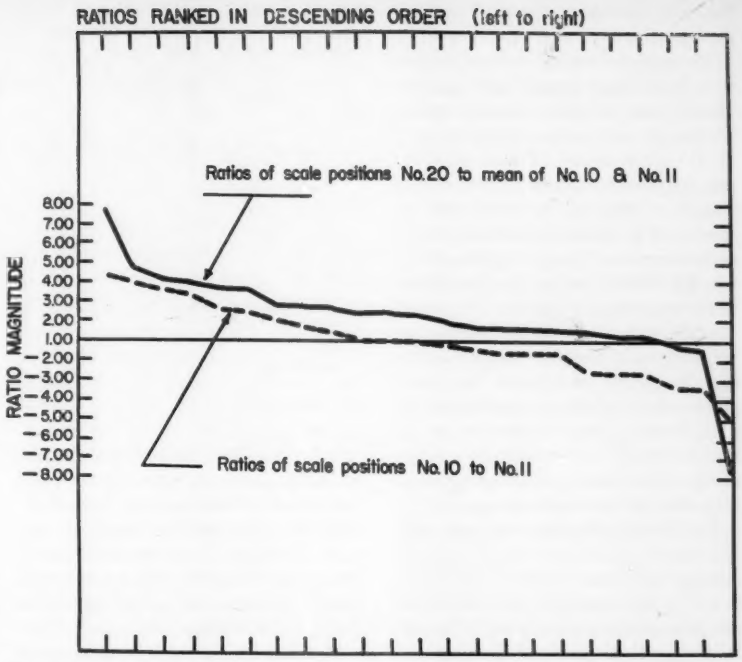

Fig. 2. Ratio distributions from the second study. Ratios of scale positions No. 10 to 11, as in the first study, are relatively restricted and quite evenly balanced above and below unity.

Table 2. Galvanic skin response ratios, arranged in order of magnitude. Neutral-to-neutral ratios are the ratios of responses to groups ranked No. 10 to responses to groups ranked No. 11. Critical-to-neutral ratios are of two kinds: positive-to-neutral and negative-to-neutral. Positive-to-neutral ratios are the ratios of responses to derogatory statements about groups ranked No. 1 to the mean of the responses to groups No. 10 and No. 11. Negative-to-neutral ratios are the ratios of responses to complimentary statements about groups ranked No. 20 to the mean of the responses to groups No. 10 and No. 11.

First study			Second study	
Neutral to neutral	Positive to neutral	Negative to neutral	Neutral to neutral	Negative to neutral
4.8	120.0	110.0	4.4	7.9
4.0	5.5	13.9	4.0	4.7
3.1	5.0	6.3	3.6	4.2
2.5	4.2	4.8	3.3	4.0
1.7	2.4	3.7	2.6	3.7
1.4	2.3	3.4	2.4	3.6
1.3	2.2	3.1	2.1	2.8
1.2	2.1	2.8	1.7	2.7
1.1	1.9	2.6	1.4	2.6
1.1	1.7	2.2	1.0	2.4
1.0	1.4	1.6	1.0	2.3
1.0	1.2	1.4	1.0	2.2
−1.1	1.1	1.4	−1.2	2.0
−1.1	1.1	1.4	−1.4	1.7
−1.3	−1.2	1.2	−1.7	1.6
−1.6	−1.6	1.1	−1.7	1.6
−1.7	−2.4	1.1	−1.7	1.5
−2.0	−3.5	1.1	−2.5	1.4
−2.4	−4.8	1.1	−2.5	1.2
−2.5	−21.2	−2.1	−2.5	1.2
			−3.3	−1.1
			−3.3	−1.2
			−5.0	−7.4

subject's attitudes prior to his coming to the laboratory.

Seventy-two college student subjects were individually measured for galvanic skin response. Nineteen did not respond sufficiently for scoring, leaving a total N of 53. Approximately 1 week after the skin response measurements had been completed, these 53 subjects were administered a paired comparison attitude scale containing the names of the groups which had been used as stimulus objects in the skin response sessions. The paired comparison scale was so devised that the name of each stimulus object (in this case, a group) was paired once with every other. A subject's preference for each stimulus object could thus be assigned a score by counting the number of times the stimulus object was selected in preference to its paired opposite.

The Keeler polygraph was again used. Individual galvanic skin response sessions differed from those of the second study in four minor ways. First, the number of complimentary statements was increased from three to nine. Second, group names were reduced from 20 to 9 and scaled by paired comparison. Third, statements were not randomized, and only three of the group names were positionally rotated from subject to subject. Fourth, the experimenter not only read the statements but operated the equipment as well.

The order of presentation of the nine statements was the same for each subject. Final balancing in was achieved by reading a preliminary statement which referred to the Irish. The names of three groups (Japanese, Jews, and Mexicans), minorities in the population from which the sample was drawn, were successively rotated from subject to subject into the statements which had positions 3, 5, and 7. It was suspected that one or another of these groups would later be scale-identified by some of the subjects as objects of strong negative attitudes. The rotation of these names was intended as a safeguard against possible positional effects.

The paired comparison attitude scale contained the following group names: Austrians, Canadians, English, Germans, Irish, Japanese, Jews, Mexicans, Poles, and Swedes. These names were randomly paired so that each appeared once with every other. Each subject circled the name of the group he preferred in each pair. The highest possible score for any group was 9 and the lowest was 0. The median test-retest Spearman rank correlation coefficient was .96.

Table 3. Galvanic skin response (GSR) and paired-comparison (P-C) ranks for stimulus groups. GSR rank 1 indicates least response by sample. P-C rank 1 indicates best liked by sample.

Stimulus group	GSR rank	P-C rank
Swedes	1	2
Canadians	2	1
Austrians	3	5
English	4	4
Poles	5	7
Germans	6	3
Japanese	7	6
Jews	8	9
Mexicans	9	8

Again, the hypothesis to be tested was: relatively strong prejudicial attitudes are supported by relatively high levels of emotion. According to the hypothesis, group name stimuli which elicit the greatest amounts of emotionality should identify those groups as objects of relatively strong prejudicial attitudes as confirmed by attitude scaling. Four analyses of the data were made.

The first analysis was restricted to data elicited by the three positionally rotated group names: Japanese, Jews, and Mexicans. By inference from the hypothesis, it was predicted that a subject whose galvanic skin response to one of these groups was above his mean galvanic skin response would rank that group below the median. The accuracy of the prediction was tested by the chi square test for two independent samples (*9*). For Japanese, Jews and Mexicans, the chi squares were, respectively, 8.54, 6.73, and 9.43. In each case $p < .005$. For most subjects, then, a relatively great skin response identified a group as relatively low on the paired comparison scale.

The second analysis dealt with data elicited by all nine group name stimuli. It was predicted that in most instances the group name stimulus which elicited the greatest skin response would be ranked below the paired comparison scale median. As it turned out, the group name stimulus which elicited the greatest skin response was ranked below the paired comparison rank median by 43 of the 53 subjects. The binomial test result was: $Z = -4.41$, $p < .001$.

The third analysis was the same as the second except for one modification. Instead of the single greatest galvanic skin response, the two greatest were identified and the same prediction was made with respect to both being ranked below the paired comparison median. Of the 53 subjects, 41 ranked both below the paired comparison scale median. The binomial test was again applied: $Z = -3.85$, $p < .001$.

The fourth analysis was directed to the relationship between the sample's attitude consensus toward the nine groups and the magnitude of galvanic skin responses which the group names elicited. The nine stimulus groups were ranked according to both skin response magnitude and paired comparison position. As predicted by inference from hypothesis, a positive relationship was found. The rank orders of these two variables are shown in Table 3. The Spearman rank coefficient was .82, $p < .01$ (*10*).

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William Fielding Ogburn, Social Scientist

Although officially classified as a sociologist, William Fielding Ogburn had far-ranging interests which carried him through all the social sciences for almost 50 years of research and teaching. Ogburn wrote in 1932, "When Wilhelm Wundt died in 1920 it was remarked that the last of the great men who knew it all had passed away. Seventeen years earlier when Herbert Spencer died he took with him a greater range of knowledge than Wundt had possessed. . . . Previous to Spencer there were many great intellects that covered the whole field of human knowledge. . . . Wundt was the last of the old men of our modern civilized tribes" [*J. Adult Educ.* (Jan. 1932)]. Ogburn, in his interests and works, must be called the last of the great social scientists who wished to know it all. It is a tribute both to the man who was able to foresee events and to the social sciences that the field has become so diversified and the total volume of knowledge so large that no one man can any longer expect to know all the social sciences.

Because of the tremendous range of his interests and the vast amount of published materials which he produced during the years 1912 (when his Ph.D. dissertation appeared) to 1959, no adequate summary or appraisal of his work is possible at this moment. As a former student and admirer of Professor Ogburn I can only hope to highlight those aspects with which I am most familiar.

He was born in Butler, Georgia, in 1886, took his B.A. at Mercer, and received his Ph.D. in 1912 from Columbia University. Between 1911 and 1918 he taught economics, political science, history, and sociology, first at Princeton University, then at Reed College and the University of Washington. He was professor of sociology at Columbia University from 1919 to 1927 and then became Sewell L. Avery distinguished service professor of sociology at the University of Chicago. He retired in 1951, at which time he began traveling about the world following his ever-broadening interests and desire to see and learn more. He lectured at the universities of Delhi and Calcutta in India and at Nuffield College, Oxford University, in England and traveled extensively through Asia, the southwest Pacific, Europe, and Latin America. Not content with a life of complete retirement, he was also visiting professor of sociology at Florida State University. Until the time of his sudden death on 27 April 1959, he was in vigorous health and continued to play tennis with anyone rash enough to face him across the net.

His research career led to his writing a number of books and hundreds of articles, most of which appeared in professional journals. No complete bibliography of his writings, which included articles in the *New York Times* and other nonprofessional journals, is available. The publication which probably attracted the most attention, both in professional and lay circles, was *Recent Social Trends* (1933), which he masterminded and directed, and of which he wrote a large portion.

Paralleling his careers as teacher and researcher was his career with the federal government. During World War I he was head of the cost-of-living section of the National War Labor Board. Subsequently he was a special agent for the U.S. Bureau of Labor Statistics, where he continued working on retail price studies for a number of years. He was a member of the Consumers Advisory Board of the National Recovery Administration in the 1930's and held posts with the Resettlement Administration and the National Resources Commission. For a number of years he was also chairman of the Census Advisory Committee.

His participation in professional societies led to his becoming president of the American Sociological Society, president of the American Statistical Association, a vice president of the American Association for the Advancement of Science, and chairman of the Social Science Research Council. None of these posts was honorary; his students can testify that he worked hard at all of these, plus many others.

Just after the turn of the century, when Ogburn began his professional career, many if not most of the leaders of the social-science disciplines were social reformers and philosophers. Against this background Ogburn brought a conviction, which was almost an obsession, that the social sciences must become scientific; they must produce verifiable knowledge. As he saw it, the use of statistical methods was one sure way of leading to scientific knowledge. In his Ph.D. dissertation he had already written, "Until it has been measured, knowledge of it must remain vague." He was greatly influenced by the anthropologist Franz Boas, the economist Wesley C. Mitchell, and the sociologist Franklin H. Giddings.

Ogburn carried this preoccupation with him during his entire life. In the textbook *Sociology* (written by him and M. F. Nimkoff) he emphasizes that a science is to be judged by three criteria —the reliability of its body of knowledge, its organization, and its method. Reliable knowledge in the social sciences can be obtained via statistical methods; statistics are the social scientsts' equivalent of the biologists' and physical scientists' laboratories. This idea was passed on to, and firmly embedded in the minds and works of, a very large number of his students in the course of a half-century of teaching. In addition to his own students, Ogburn must have influenced untold thousands of students who received their introduction to the subject of sociology through the text *Sociology*; for over a decade this was the most widely used introductory text.

His emphasis on obtaining scientific—that is, verifiable—knowledge, led to the writing of an admirable paper [*Social Forces* **8**, No. 3] in 1930, which is as fresh and illuminating today as it was a generation ago. The first obstacle to the development of a scientific sociology, he argued, is intellectualism. "Scholarship and science are different . . . The scientific process of discovering new knowledge usually consists of two steps, getting an idea or hunch and proving it after having formulated it into an hypothesis. Intellectualism encourages greatly the first step in this process. . . . But it greatly hinders the second step in the process. And it must be remembered that ideas are not knowledge, and they can be

proven to be knowledge or not only by laborious effort. Until that is done we do not have a science."

This emphasis, also, prevented him from ever attempting to set forth a cosmic scheme that would explain all human behavior, and he had little use for all the writers (including those of today) who developed broad theories in the absence of (or in disregard for) verifiable knowledge. Having some of these cosmic writers in mind, Ogburn (and Nimkoff) wrote: "Some of the organizations make very beautiful systems . . . These systematizers think they are setting up guides which the younger scientists, those who follow them, will use. But often these systems are futile, as was the organization of medicine around the theory of convulsive action advanced by Benjamin Rush, a century ago" [*Sociology* (Houghton Mifflin, Boston, 1950), p. 18].

The maximum amount of theorizing that Ogburn permitted himself was the idea that culture is an interrelated matrix of events. Virtually every element affects every other element. The interrelationships are all highly complex, and the various elements are related to each other in different degrees. Thus, a large change in one element might lead through a highly complex matrix of interrelationships to only a small change in another element. For example, a very large change in the extent to which a population is urbanized may lead, eventually, to only a small change in the proportion of the population which is married. Furthermore, these interrelationships do not all work as automatically as the gears in a watch. Opposing interrelationships come into play; for example, people driving motor vehicles are the cause of around 40,000 deaths a year in the United States, but when used as ambulances, motor vehicles also save lives. Since so many of these interrelationships are unknown, however, he believed that the time was not yet ripe to try to weave them all together into an over-all theory of human behavior.

William Fielding Ogburn

The nearest he came to formulating such an over-all theory was in his volume *Social Change* (1922). Here he attempted to weave together information from biology, anthropology, psychology, economics, and sociology. Full discussion of this book is beyond the scope of this article. It can only be said that he emphasized that changes in the social heritage of mankind are due to factors in human society rather than to any biological changes in man himself.

In trying to account for social change, Ogburn selected technology and invention as a prime mover. He recognized that other factors, including ideas, could effect changes, but he chose to study inventions. As much as he would have liked to study everything, he realized that he could not do so, and hoped to obtain some scientific knowledge about social change by limiting his research to the factor of technology and inventions. He wrote: "Invention is a great disturber and it is fair to say that the greatest general cause of change in our modern civilization is invention; although it is recognized that social forces in turn encourage or discourage inventions. Certainly developments in technology cause a vast number of changes in a great variety of fields" [*Technological Trends and National Policy* (National Resources Committee, June 1937), p. viii]. Altogether he wrote several books and many journal articles on technology as a factor in social change.

Professor Ogburn was always the gracious southern gentleman, courteous, even-tempered, and judicious. These personal qualities not only endeared him to all who knew him but led to his becoming the statesman of the social sciences—the scholar who could best present the case of the social scientist to the world of nonscientists.

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Bureau of Applied Social Research, Columbia University, New York

News of Science

Scientist Exchange Pact Signed by Nesmeyanov and Bronk

During this year and next the academies of science of the United States and the Soviet Union will exchange research scientists for periods of up to 6 months under a new pact signed on 9 July by Detlev Bronk, president of the National Academy of Sciences, and Alexander Nesmeyanov, president of the Academy of Sciences of the U.S.S.R. The agreement, which represents the first time that an exchange of scientists has been sponsored by the U.S. and Soviet national academies, calls for lectures, research work, and observation of scientific studies by the scientists involved in the exchange, and for the organization of symposia on current scientific problems by the two academies. Approximately 90 researchers will take part in the 2-year program. The treaty provides for possible continuation of the program after the first 2 years.

The diplomatic basis for the new pact was established in 1958 when the Lacy-Zaroubin agreement, calling for cultural, technical, and educational exchanges between the two countries, was signed. That agreement gave the academies of science of the signatory countries responsibility for effecting certain of these exchanges. Other exchanges have been arranged by various governmental and nongovernmental groups; the U.S. tour of the Bolshoi Ballet, the cultural fairs in Moscow and New York, and exchanges of agricultural experts are examples.

The agreement calls upon each academy to name individuals to participate in exchange visits in the three following categories: (i) approximately 20 noted scientists from each country "to deliver lectures and conduct seminars on various problems of science and technology as well as for the purpose of studying research work in progress" during visits of up to 1 month; (ii) approximately 18 scientists from each country to spend 1 month in laboratories of the opposite country observing current research in 14 designated scientific specialties; (iii) an additional six scientists from each country to spend longer periods in specialized fields of study or in the conduct of research in six designated scientific specialties.

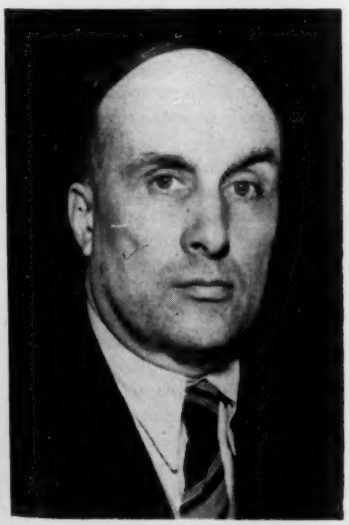
The agreement stipulates that each side is to pay the salaries of its own scientists and their travel expenses between the two countries; the host country will be responsible for the cost of living quarters and medical aid, and for certain local travel expenses. The U.S. Academy has been assured of sufficient funds from the National Science Foundation and other public and private sources to defray all expenses of the U.S. half of the program.

Publications Exchange Planned

In addition to exchange of personnel, the pact calls for an exchange of scientific publications between the academies. Another article of the treaty provides for the invitation on a reciprocal basis of scientists of one country to the important congresses, conferences, and meetings of the other. Exchanges in these areas have occurred in the past, but, as a result of the treaty, they are now matters of official policy of the two academies.

Prior to the signing of the agreement, both academies submitted lists of the type of research that would be of particular interest to their visiting members. Among the projects in which the Soviet academy expressed interest were studies concerned with spectroscopic apparatus, automatic control and information theory, photosynthesis, radioastronomy, and the biochemistry of cancer. The National Academy of Sciences cited studies in probability processes, cosmic rays, weather prediction, and the physiology and biology of the nervous system as matters of particular interest to its members.

Although the Lacy-Zaroubin agreement allows for visits of up to 1 year, the longest period mentioned in a tentative list of projects released by the National Academy of Sciences is 6 months. This period is given for exchange visits in the third category—those which will allow 6 scientists from each country to work with foreign colleagues in research on specific problems. Fields of study for these longer visits are, for Soviet researchers, radiospectroscopy, stereochemistry, high-molecular compounds, theory of metallurgical processes, information theory, and biochemistry. American researchers plan to work with Soviet colleagues in studies of solar physics, nonlinear mathematical systems and differential equations, conditioned reflexes, the physiology of stress, celestial mechanics, and the physical chemistry of high polymers.



Alexander Nesmeyanov, president of the Academy of Sciences of the U.S.S.R.

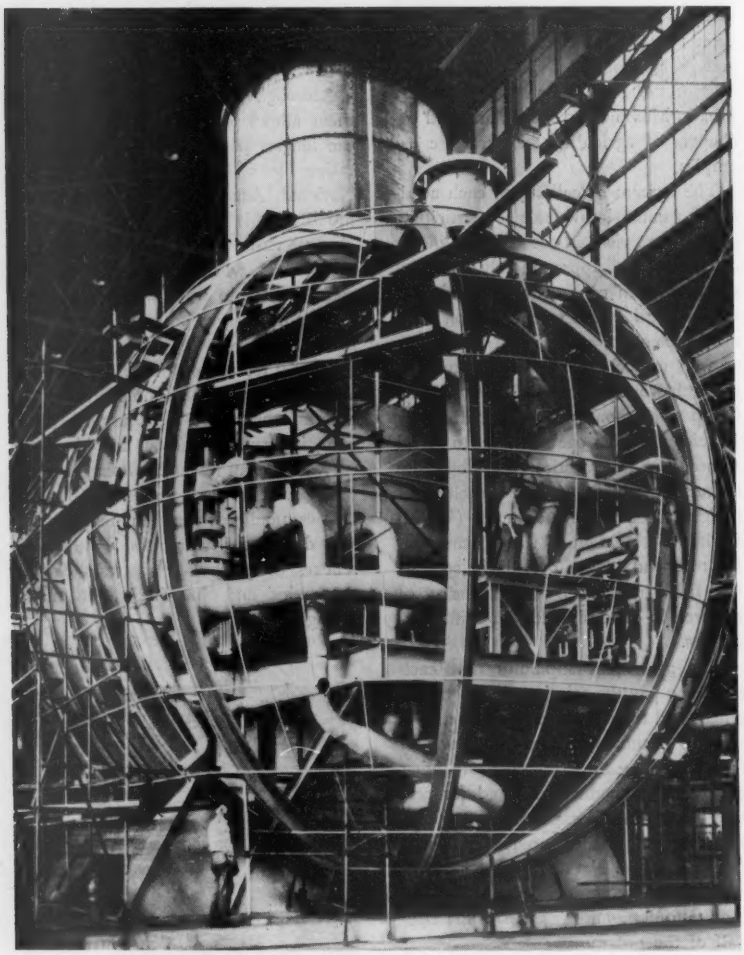
Detlev Bronk, president of the National Academy of Sciences of the United States.

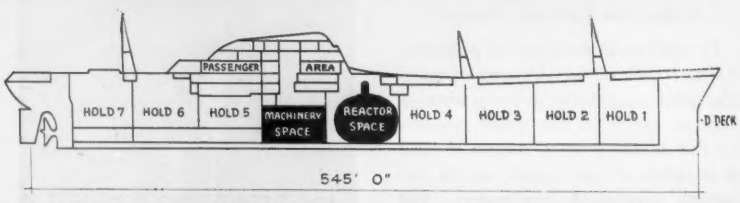
Nuclear Ship "Savannah" Launched

In ceremonies at Camden, N.J., the N.S. *Savannah,* the world's first nuclear-powered passenger and cargo ship, was launched in the Delaware River. Mrs. Eisenhower christened the ship. Work on the vessel, which is about 70 percent complete, will continue; the concrete shielding has still to be poured and the nuclear propulsion system installed.

The building of the *Savannah* was undertaken, in line with administration and congressional policy, to develop the U.S. merchant marine and to demonstrate to the world the United States' intention to employ the power of the atom for peaceful, productive purposes.

The design and construction of the ship are the joint responsibility of the Maritime Administration of the U.S. Department of Commerce and of the U.S. Atomic Energy Commission. The combined effort is being made through a joint group known as the Nuclear Projects Office in the Maritime Administration, and as the Maritime Reactors Branch in the Atomic Energy Commission. The vessel was designed by George G. Sharp, Inc. The Babcock and Wilcox Company was awarded the principal contract for developing and manufacturing the propulsion machinery.





(Top) Full-scale mock-up of the nuclear power plant of the N.S. *Savannah* and (bottom) position of the actual reactor in the completed ship. In the mock-up the containment vessel of the reactor is outlined in skeletal form to permit a clear view of the interior. The reactor is approximately 70 feet long and 55 feet high.

The building of a nuclear-powered merchant vessel was first proposed by President Eisenhower in a speech in New York on 25 April 1955. Construction was authorized by Public Law 848, on 30 July 1956. States Marine Corporation of Delaware will operate the ship as general agent for the Maritime Administration.

The contract with the Babcock and Wilcox Company was signed 4 April 1957; the contract for construction of the vessel was signed with the New York Shipbuilding Corporation, Camden, N.J., on 15 November 1957; the keel was laid on National Maritime Day, 22 May 1958.

Construction of the vessel will be completed by early 1960. It is expected that the *Savannah* will undergo extensive testing next spring and that she will be ready for unrestricted operation by next summer.

From the first it has been acknowledged that the *Savannah* will not be able to compete economically with passenger and cargo ships powered by traditional means, and she is not intended to be a prototype. Rather, the objective in building this "first generation" nuclear-powered merchant vessel was to develop practical construction and operating techniques which could be evaluated and used in designing "second and third generation" nuclear-powered ships which would provide real economic competition.

The *Savannah* will be a test ship. Many special features, such as extensive means of controlling operational components from a distance and means of achieving greater maneuverability than is essential to a merchant vessel, have been incorporated for evaluation. It is expected that, during operation of the *Savannah,* changes will be made in components, and even in entire plant systems, when it seems probable that significant improvements can be made.

The *Savannah,* according to its designers, has five important missions: (i) to demonstrate to the world the employment of nuclear power in an instrument of peace; (ii) to utilize the power of the

atom for trade and commerce; (iii) to demonstrate that nuclear-powered ships are dependable and safe; (iv) to stimulate study of such problems as international liability and legal security against accidents and to win acceptance of nuclear ships in the world's ports; and (v) to give the Maritime Administration and the Atomic Energy Commission opportunity to assess the potential contributions of atomic power to American commerce.

Seven NATO Allies To Get Atomic Data from U.S.

On 11 August the last of seven atomic cooperation pacts between the United States and certain of her NATO allies will go into effect. The agreement, through which Greece will receive from the United States training, equipment, and information which will enable her to use and to defend herself against nuclear weapons, is similar to other pacts the U.S. signed last May with Canada, Turkey, West Germany, and the Netherlands. Related agreements with a somewhat different purpose were made at the same time with Britain and France. These atomic cooperation agreements, which were among the last official actions of the late John Foster Dulles, automatically became effective during the last half of July. They had been signed 60 days earlier in Bonn, Ankara, and The Hague and in the capitals of other NATO countries, but the U.S. Congress, before approving them, made provision for a 2-month interim period to permit further deliberation on the part of its members. During the 60-day period the pacts could have been repudiated by the passage of a concurrent, or combined, House and Senate resolution. No such action was taken, and there was almost no debate on the pacts. Senator Hubert Humphrey (D-Minn.) raised questions about the agreements on the Senate floor, and five Representatives spoke against them in the House. There was, however, no organized opposition, and on the effective dates six of the pacts become operative. Four days from now the last one—the Greek-U.S. agreement, will be established.

Under the pacts, the U.S. will provide the signatory countries, other than Britain and France, with the means to train personnel in the use of atomic weapons and in defense measures; with information which will enable these countries to develop defense plans and systems for delivering atomic weapons; and with nonnuclear components of atomic weapons systems, the "gear" that is used to attach a nuclear warhead to a missile or a plane. Under the agreement the U.S. will not furnish information on the design of the weapons themselves, or on the electronic devices that are incorporated in them.

The agreement with Britain differs significantly. In accepting the pact proposals Congress specified that information on design be given only to nations which had made "substantial progress" toward the manufacture of atomic weapons. Britain alone qualifies under this stipulation, and she will receive information on design, materials, and other matters relevant to bomb construction. France will receive information on constructing power plants for atomic submarines together with about half a ton of enriched uranium to fuel the prototype of such a plant.

Spurred by Sputniks

The NATO pacts which have just become effective had their origin in the conferences that followed the launching of the first Soviet satellite, on 4 October 1957. At those conferences it was decided that "an enlarged Atlantic effort in scientific research and development" was necessary to meet the challenge of the sputniks. At a later meeting of the North Atlantic Council, in December 1957, it was further decided that atomic weapons should be stockpiled in certain NATO countries and that systems for delivering them should be developed in those countries.

At the time this decision was made, the laws of this country, specifically the Atomic Energy Act of 1954, prevented the United States from cooperating to the extent the NATO leaders felt necessary. This law was amended, and, with the fulfillment of the pact with Greece, the sharing of American atomic defense information with NATO allies has now become a reality.

Williams Named for AEC Post

John Harry Williams, a physicist at the University of Minnesota, has been named by President Eisenhower to succeed Willard F. Libby as a member of the Atomic Energy Commission. Williams, who has been director of the commission's programs of basic research, will be the scientist on the five-man commission. It has become customary in the last 12 years to have at least one scientist among the five commissioners. The nomination, which was announced 16 July, must be confirmed by the Senate. Quick, favorable action is expected because the nomination was informally approved by the members of the Joint Atomic Energy Committee of the Congress before the announcement was made.

Most of William's teaching and research career has been at the University of Minnesota, where he has been a professor of physics since 1946. During World War II he participated in the development of the atomic bomb at the Los Alamos Laboratory in New Mexico.

Williams was born in Asbestos Mines, Quebec, Canada, on 7 July 1908. He was graduated from high school in Kelowna, British Columbia, and received his B.A. degree from the University of British Columbia in 1928. As a teaching fellow at the University of California he received his master's degree in 1930, and as a Whiting fellow, received his Ph.D. in physics in 1931. From 1931 to 1933 he was a National Research fellow in physics at the University of Chicago. He joined the University of Minnesota as a physics instructor in 1933, became assistant professor in 1934, associate professor in 1937, and professor on his return from Los Alamos. In 1942 he became a naturalized citizen of the United States.

Radioisotope Training Program

The Atomic Energy Commission has announced a new program which will give students of small undergraduate colleges opportunity for specialized training in the techniques of using radioisotopes. The program will utilize a 35-foot, bus-type mobile training laboratory, which can be moved to the college campus for presentation of a two-week, concentrated course on the basic techniques of handling radioisotopes. The mobile laboratory will be similar to one presented last year to the International Atomic Energy Agency by the United States.

The program is expected to begin in the fall of 1959, and initially the course will be available to colleges in the South and Southeast. The project is designed to serve in particular the smaller colleges whose nuclear facilities are limited.

Scientists and technicians from the Oak Ridge Institute of Nuclear Studies, which will administer the program for

the commission, will accompany the mobile radioisotope laboratory to the colleges to lecture and direct experimentation. The course will be a condensed version of a basic course which has been conducted at Oak Ridge by the institute since 1948, to train several thousand research workers from the United States and from many countries abroad.

Loyalty Oath Provision of Education Act to Remain

The Senate, on 23 July, voted 49 to 42 to recommit to one of its committtees a bill that would have amended the loyalty oath provision of the National Defense Education Act of 1958. The action effectively precludes any further action this session on the controversial oath and disclaimer clauses and any attempts to eliminate or modify them.

Passed last year, the National Defense Education Act requires that a student seeking an educational loan from the government must take the oath of allegiance and submit an affidavit, disclaiming membership in, or support of, any organization advocating illegal overthrow of the government. Objection to these provisions, which many feel imply that students are "particularly suspect," has been voiced by numerous academic groups. During debate on the issue, Senator John Kennedy (D-Mass.) argued for the repeal of both provisions. This proposal and others modifying the controversial provisions were defeated in the voting.

Survey of Soviet Science Literature

A detailed survey of the present effort to supply United States scientists with Russian scientific literature was released 19 July by the National Science Foundation; it lists 76 Soviet journals now available in English. The survey reports on the sources of Soviet scientific literature, the availability of such literature in the United States, and the current translation programs of professional and academic groups and government agencies. Current methods of providing comprehensive covering of untranslated Russian material are also analyzed. Revised and expanded from an earlier edition, the survey, *Providing U.S. Scientists with Soviet Scientific Information,* was prepared by the foundation's Office of Science Information Service.

According to the survey, 2026 scientific journals and serials are being published in the U.S.S.R. Since 1956, Soviet scientific publications have become increasingly available in the United States. They are listed in the *Monthly Index of Russian Accessions,* published by the Library of Congress and available from the Superintendent of Documents of the Government Printing Office. Many, although not all, publications of individual institutions in the Soviet Union are available on an exchange basis, the survey states. The processing department of the Library of Congress maintains a list of Soviet institutions with which the library exchanges publications on an exchange basis.

Translations of Soviet scientific journals are made available by three methods. (i) A large number of translations are made under grants by federal agencies to nonprofit scientific societies or research institutions. Most National Science Foundation grants are of this type. (ii) Other translations are produced under government contract with translating agencies. (iii) A number of publications are translated by individuals or private organizations, without governmental subsidy.

The two main translation depositories in the United States are listed as the Special Libraries Association Translation Center, located at the John Crerar Library in Chicago, which collects translations from nongovernment sources, and the Office of Technical Services, Technical Information Division, Department of Commerce, which collects from federal agencies and foreign sources.

All translations are listed, with abstracts, in *Technical Translations,* published bimonthly since January 1959 by the Department of Commerce. Translations of Russian scientific books are made by commercial publishers, by government agencies through contract or grants, or directly by the agencies themselves.

According to the survey, translation programs in the United States have tended to provide cover-to-cover translation of selected journals and abstracts rather than to select individual papers for translation. To provide full translations in broad subject fields has been found preferable, to guard against inadvertent omissions and to facilitate bibliographic handling. An economic study of this approach has been made, and the results show that even if only one paper in 40 is of general interest, it is cheaper to translate all 40 than to determine which one to translate.

News Briefs

The extension of U.S. "ocean" ports as far inland as Duluth, Minn., by the opening of the St. Lawrence Seaway has presented new problems for plant, livestock, and meat regulatory workers of the U.S. Department of Agriculture. Discovery by plant quarantine inspectors of khapra beetles, pests that destroy stored grains, aboard a freighter putting in at Cleveland, Ohio, is an example of this new problem in combating foreign plant and animal pests. The tiny khapra beetle, a native of the Far East, has invaded the U.S. only in the Southwest. An intensive federal-state fumigation effort is in progress to eradicate it there. The Cleveland finding placed the pest unpleasantly close to the grain belt of the Midwest.

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An open-pool reactor, the first to be built in the Philippines, is under construction at the University of the Philippines at Diliman, Quezon City, 8 miles northeast of Manila. The reactor will produce 1000 thermal kilowatts initially, and its design will allow for expansion and production later of 3000 thermal kilowatts. It is scheduled to be placed in operation by the fall of 1960. Purchase of the reactor was made possible by a $500,000 economic grant from the U.S. Atomic Energy Commission, made available under President Eisenhower's "Atoms for Peace" program.

* * *

A report on the watershed problems of New England's forested areas has recently been published by the Northeastern Forest Experiment Station. The report, "A Problem Analysis and Program for Watershed-Management Research in the White Mountains of New Hampshire," describes the problems faced, the research needed, and the priorities for various study projects. Copies of the report are available from the Northeastern Forest Experiment Station, 102 Motors Avenue, Upper Darby, Pennsylvania.

* * *

The John A. Hartford Foundation reports that during 1958 it appropriated $5,585,066 for 66 new or continuing grants to medical institutions. The first formal report in the organization's 30-year history shows that in the 7 years following the founder's death in 1951, the foundation made grants and gifts totaling more than $18 million. The report listed the foundation's assets at more than $166 million at the start of this year.

Scientists in the News

H. TRENDLEY DEAN has retired as secretary of the council on dental research of the American Dental Association. His pioneer research on the effects of adding fluoride to city water supplies made him internationally known as a specialist on fluoridation. Before joining the association in 1953 he was associated for more than 30 years with the U.S. Public Health Service, serving for a time as director of the National Institute of Dental Research. He is succeeded by SHOLOM PEARLMAN, who has been assistant secretary of the association's council on dental therapeutics since 1951.

LEMUEL R. CLEVELAND, professor of biology and an outstanding parasitologist, known for his studies of protozoa, is retiring from Harvard University. He received his B.S. degree from the University of Mississippi in 1917 and his Sc.D. degree from Johns Hopkins University. He joined the staff of the Harvard Medical School in 1925 as assistant professor of protozoology. Dr. Cleveland has been professor of biology since 1946.

KARL F. HEUMANN, former research director for *Chemical Abstracts,* has accepted appointment as director of the newly established Office of Documentation at the National Academy of Sciences–National Research Council. Among his duties will be that of advising NAS–NRC groups and some federal agencies on documentation matters.

DAVID A. YOUNG, formerly assistant chief engineer of the liquid engine division of Aerojet–General Corporation, Azusa, Calif., has been appointed director of the corporate long-range planning division.

FRANCIS A. GUNTHER, insect toxicologist at the University of California, Riverside, has received the 1959 Harvey W. Wiley Award of the Association of Official Agricultural Chemists. The award of $500 was established in 1956 to honor Wiley, father of the original pure food and drug law and a founder of the association.

GEORGE W. MOREY, physical chemist with the U.S. Geological Survey, will be awarded the Howard N. Potts Medal by the Franklin Institute, Philadelphia, Pa., on 21 October.

JOHN C. MARR, former director of the La Jolla (Calif.) Biological Laboratory, U.S. Fish and Wildlife Service, has been appointed director of the Honolulu (Hawaii) Biological Laboratory.

S. P. YANG, assistant professor of foods and nutrition at Purdue University's School of Economics, will lecture at the National Taiwan University, Taipei, from September 1959 to January 1960.

RICHARD P. FEYNMAN has been named Richard Chace Tolman professor of theoretical physics at California Institute of Technology. The newly created chair honors the late Richard Tolman, an internationally known theoretical physicist and chemist who was dean of graduate studies at the institute.

GEORGE W. ALBEE, chairman of the psychology department and associate professor of psychology at Western Reserve University School of Medicine, has been appointed George Trumbull Ladd professor of psychology. WALTER LEIGHTON, professor of mathematics in the College of Engineering and Science at Carnegie Institute of Technology, has been appointed Elias Loomis professor of mathematics and chairman of the department at Western Reserve University.

LINWOOD H. WARWICK, geological survey assistant with the Division of Mineral Resources, Department of Conservation and Economic Development of the state of Virginia, retired on 30 June after having been affiliated with the Division for more than 51 years.

WILLIAM W. GREULICH, professor and head of the department of anatomy, Stanford University School of Medicine, has been appointed president of the American Association of Physical Anthropologists.

ROBERT A. TERRY, geologist, has been awarded the Republic of Panama's highest award, the Order of Vasco Nunez de Balboa, for outstanding geologic work in Panama.

F. W. G. WHITE, deputy chairman of the Commonwealth Scientific and Industrial Research Organization, Melbourne, Australia, has been appointed chairman of that organization. He succeeds Sir IAN C. ROSS, who died on 20 June.

THEODORE E. STERNE, former associate director of the Astrophysical Observatory of the Smithsonian Institution and Simon Newcomb professor of astrophysics at Harvard University, has joined the staff of the Johns Hopkins University Operations Research Office, Bethesda, Md.

ROBERT C. TRUAX, captain, U.S. Navy (ret.), and former national president of the American Rocket Society, has been appointed director of advanced development at the Aerojet–General Corporation's Liquid Rocket Plant, Sacramento, Calif.

ROBERT H. HAMLIN, assistant to the secretary of the U.S. Department of Health, Education, and Welfare, has resigned to become associate professor of public health administration in the department of public health practice, Harvard University.

ROY C. SWAN, associate professor of physiology at Cornell University Medical College, has been appointed chairman of the department of anatomy.

Recent Deaths

CLEMENT S. BRAININ, Glen Ridge, N.J.; 72; founder of the C. S. Brainin Co., Mount Vernon, N.Y.; formerly taught astronomy at Columbia University; 20 July.

BRONSON CROTHERS, Cambridge, Mass.; 75; clinical professor of pediatrics at the Harvard Medical School; 17 July.

E. NEWTON HARVEY, Princeton, N.J.; 71; Henry Fairfield Osborn professor emeritus of biology of Princeton University; former vice president of the Marine Biological Laboratory, Woods Hole, Mass.; noted for his research on bioluminescence; 21 July.

HAROLD V. O'CONNELL, New York; 53; dentist and inventor of two devices for dental anesthesia; 17 July.

ALEXANDER V. TOLSTOOUHOV, New York; 70; chemist and toxicologist; former senior investigator of Warner-Lambert Research Institute, Morris Plains, N.J.; before coming to this country from the U.S.S.R. in 1923 was a faculty member at universities in Poland and Russia; 9 July.

EMMA L. WARDELL, Baltimore, Md.; 72; retired toxicologist of the Chemical Warfare Laboratories, Army Chemical Center, Md.; 8 July.

Book Reviews

The Sleepwalkers. A history of man's changing vision of the universe. Arthur Koestler. Macmillan, New York, 1959. 624 pp. Illus. $6.50.

In the preface to this book, Koestler notes with some justifiable astonishment that there is no modern history of cosmology, "no comprehensive survey of man's changing vision of the universe which encloses him." His own book, to judge from its subtitle, while intended to fill this gap, is nevertheless, as it turns out, no such complete history. For while it opens with the Babylonians and ends with Newton, the bulk of it is actually devoted to an account of the three titans of modern planetary astronomy—Copernicus, Kepler, and Galileo. In recounting the story of their lives, the pattern of their discoveries, and their impact upon the course of modern intellectual development, Koestler would serve a double purpose. He would not only retrace the history of one of the most fascinating and momentous periods in science; he would also draw the moral that is to be found in reflecting upon that history. For him that moral is that the allegedly common notion of the history of science as a steady, cumulative affair must be reassessed and discarded, and that emphasis must be placed on the mutually supplementary roles of science and mystical religion in any complete intellectual approach to the universe. What emerges is a lively, provocative, yet markedly uneven book. One suspects that the book began as a biography of Kepler, of whom no satisfactory account had yet been given in English, and was gradually built up to include an account of pre-Keplerian cosmological speculations and of Kepler's immediate predecessors and contemporaries. Indeed, since the presentation of the cosmology of the ancient and medieval world is quite inadequate, and the treatment given Copernicus and Galileo shows much hostility and bias, the book would have had a greater evenness and unity if it had remained precisely such a study of Kepler. For it is in studying Kepler, his hero, that Koestler finds occasion for close study of the theme that interests him most. Kepler is an outstanding example of the type of individual (of which Pythagoras was the prototype) in whom are inextricably combined the mystic and the savant. Moreover, in his major works Kepler has left us a carefully documented account of the processes of his own creative thinking, with all its attendant blundering, dogged persistence, and heartbreak. In the life of this one man, Koestler finds summed up the whole history of cosmic theories, for that history "may without exaggeration be called a history of collective obsessions and controlled schizophrenias; and the manner in which some of the most important individual discoveries were arrived at reminds one more of a sleepwalker's performance than an electronic brain's."

That the subject matter of cosmology should engage the attention of Arthur Koestler might, at first glance, occasion some surprise. One tends to think of him as the author of novels with political themes—in particular, of *Darkness at Noon.* Yet, when we remember his other books, such as *Insight and Outlook* and *The Yogi and the Commissar,* we realize that the interest there shown in the various levels of psychological awareness and in the reconciliation of empirical and mystical attitudes is what here, in *The Sleepwalkers,* continues to manifest itself, though in a fresh context.

The cosmology that dominated men's minds throughout the ancient and medieval world was rooted in a number of dogmas whose unshakable hold over men's minds proved the most serious obstacle to any fruitful advance in physics and astronomy. Chief among these was the belief (inherited from Aristotle) in two sets of physical principles, one for terrestrial phenomena, the other for celestial. A second was the belief in the uniquely centered and privileged position of the earth in a spatially finite, spherical universe—the dogma of geocentrism. A third was the demand that all explanations in geometric terms of the motions of the heavenly bodies be construed in terms of circular motions. This bit of apriorism, established by the Pythagoreans and confirmed by Plato, was to saddle mathematical astronomy from the days of Eudoxus to those of Copernicus. What we call the scientific revolution, on its cosmological side largely initiated by Copernicus and brought to a relatively final synthesis by Newton, consisted in a gradual loosening and abandoning of each of these dogmas and their replacement by views that had greater empirical justification and theoretical usefulness.

Copernicus, to whom Koestler refers as "the timid Canon," was, in his own person and mentality, far from being a scientific revolutionary. The author of *The Book of the Revolutions of the Heavenly Spheres,* according to Koestler "was denied the essential qualities of the prophet; awareness of a mission, originality of vision, the courage of conviction." The long delay in publication of *The Book of Revolutions* was caused not by fear of condemnation by ecclesiastical authorities but by fear of ridicule. Copernicus was indeed a conservative in the major lines of his thinking. Of the principal doctrines defining orthodox cosmology summarized above, Copernicus left untouched the adherence to Aristotelian physics and the method of circular description for celestial motions. At a time when men like Roger Bacon, Nicholas of Cusa, William of Occam, and Jean Buridan had already seriously weakened the hegemony of the older physics, Copernicus cleaved to the older scheme. In his continuance of the use of epicycles and deferents, the mainstay of Ptolemaic astronomy, Copernicus in no way departed from the ancient tradition. As a matter of fact, as Koestler points out, the oft-repeated claim that Copernicus reduced the number of epicycles required in planetary astronomy of the Ptolemaic type is actually false. Although Copernicus had claimed in the *Commentariolus* that "thirty-four circles suffice to explain the entire structure of the universe and the entire ballet of the planets," this was but an optimistic preliminary announcement that was not matched by the actual performance set forth in the *Revolutions.* The Ptolemaic scheme, as brought up to date by Peuerbach in the 15th century, required but 40 circles, yet Copernicus in his own sys-

tem requires 48; thus, far from reducing the number of circles, he actually increased them. Where then was the great significance of his work? It was, of course, in the shift he accomplished for astronomy and cosmology from a geocentric to a heliocentric orientation. He demolished that pillar of the older cosmology by offering a tremendously simplified explanation of the *retrograde* motions of the planets. "So long as the earth was the hub of the universe, this phenomenon could be 'saved' by adding more epicycles to the clockwork, but there was no natural reason why the planets should behave as they did. But if the hub was near the sun, and the earth turned round it together with the other planets, it was obvious that each time the earth 'overtakes' one of the outer planets (which circle at a slower rate) that planet will appear to recede for a while; and each time the earth itself is overtaken by the faster moving inner planets, an apparent reversal of direction will again result." It was this idea that provided the impetus for the new way of thinking that flowered into the Copernican revolution and that brought with it the eventual shattering of the older cosmology.

It was Kepler's role in this revolution that engages the full and admiring attention of Koestler as he relates the incidents and guiding motifs of the life of that tortured genius. The section of Koestler's book dealing with Kepler is the richest and most rewarding part. Koestler's pen traces in fascinating detail, with the aid of Kepler's painstaking autobiographical remarks about his work and personal affairs, the pattern of a fiercely dedicated life. "Johannes Kepler, Keppler, Kheppler, or Keplerus was conceived on 16 May, A.D. 1571, at 4.37 a.m. and was born on 27 December at 2.30 p.m., after a pregnancy lasting 224 days, 9 hours and 53 minutes. The five different ways of spelling his name are all his own, and so are the figures relating to conception, pregnancy and birth, recorded in a horoscope which he cast for himself. The contrast between his carelessness about his name and his extreme precision about dates reflects, from the very outset, a mind to whom all ultimate reality, the essence of religion, of truth and beauty, was contained in the language of numbers." At the age of 24 Kepler received an overpowering inspiration that was to dominate and guide him in all his work for the rest of his life. Kepler carefully recorded the date of this inspiration—9 July, 1595, while he was drawing a figure on the blackboard for his class. It was then that the idea struck him—which he regarded as the key to the secret of creation—that the universe (or, more precisely, the distances between the six planets known in his day) was built around the five perfect or Platonic solids. The idea was, of course, a completely false one. Yet, as the main inspiration for his first book, the *Mysterium Cosmographicum,* published when he was 25, it was nevertheless, like so many other erroneous thoughts, the breeding place of many fruitful ones. As Koestler remarks, "it led eventually to Kepler's laws, the demolition of the antique universe on wheels, and the birth of modern cosmology."

Underlying this inspiration and the prolonged search for the exact mathematical patterns that bind together the planetary world—patterns that Kepler, as a true empirical scientist, insisted must square with the observational materials at hand—was a basically religious outlook. He believed that the sun is at the center of the world because it is the symbol of God the Father, the source of light and heat and of all moving energy. The sphere of the fixed stars he took to be the symbol of the Son, while the invisible forces which, emanating from the Father, act throughout interstellar space represent the Holy Ghost. And binding all these together is the unifying scheme of Geometry, which God used in creation and which the mind of man can hope to discover. The prolonged search for the laws of planetary astronomy that marked Kepler's life as a scientific quest is of the greatest value for the student of scientific methodology. To the probing eye of the psychologically oriented biographer, the growth of the man is of equally great fascination. Koestler suggests that "the sufferings of a mange-eaten, chaotic childhood had left a sober thirst for universal law and harmony; memories of a brutal father may have influenced his vision of an abstract God, without human features, bound by mathematical rules which admitted of no arbitrary acts."

Koestler's estimate of Galileo, the third of the great figures treated in this book, is in sharp contrast to his estimate of Kepler. He finds the personality of Galileo unattractive. He would counteract the wide acceptance of Galileo's importance in the history of astronomy, while giving him full credit as the founder of modern dynamics. The trial of Galileo, according to Koestler, should not be taken as an example of the inevitable conflict in the philosophies of blind faith and enlightened reason, of Church and science. Rather, it was "a clash of individual temperaments aggravated by unlucky coincidences."

The historical portion of the book is brought to a close with a brief chapter on the Newtonian synthesis. Koestler feels it quite appropriate to end his survey at this point. Dismissing all the most recent and highly significant researches of modern physics and cosmology as still largely too controversial, he would have us believe that "the blueprint of the universe remains essentially the one Newton drew for us, in spite of all disturbing rumours about the curvature of space, the relativity of time, and the runaway nebulae." The judgment just quoted is so contrary to the most obvious truth and, in its own way, so perverse, that it almost seems as if Koestler uses it as a rationalization for choosing the stopping place he did choose for his own historical review.

Koestler's dim view of the intellectual worth of contemporay science is based on its allegedly paradoxical character and unintelligibility. The fundamental reason for his disparagement is that, in his opinion, modern science has become isolated from the inspiration which a religious outlook on the world has to offer, and did offer earlier thinkers. It has become a mere technical and abstract manipulation of symbols. Such symbols are solely useful as devices for "saving the phenomena" but are useless for giving us insight into the real structure of the universe. The divorcement of science from a faith in the rationality of the universe, in its underlying purposiveness and orderliness, has resulted in a form of scientific *hubris* which may prove to be man's undoing. The spiritual ebb tide of the modern age coexists with a unique increase in physical power and therefore poses the greatest dangers, from the potential misuse of that power. Unfortunately, Koestler's indictment of modern science lacks convincingness, since what he offers as a corrective is itself so mistily and narrowly conceived as to be philosophically of little value. That the religious dimension of experience or the mystic's perspective on the world are in themselves important to any fully developed mentality need not be challenged. What needs to be challenged is Koestler's belief that only with some faith in the purposive orderliness of the world can science achieve the drive to sustain its inquiries. Religion or

mysticism need not be so conceived that some ontological teleology is essential to their viewpoints. It is sufficient if a deeply cultivated sense of the mystery of existence provides a corrective to any dogma of the total intelligibility of the universe, an intelligibility to be finally revealed by science or any other means.

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Federal Budget and Fiscal Policy, 1789–1958. Lewis H. Kimmel. Brookings Institution, Washington, D.C., 1959. x + 387 pp. $5.

This highly readable volume has a somewhat misleading title. Kimmel has not written a comprehensive history of federal budgeting and fiscal policy but has concentrated on the evolution of attitudes toward the balanced budget and public debt. The volume starts with Washington and Hamilton and ends with Eisenhower and Anderson. Economic events, the viewpoints of government officials, and the contemporary prescriptions and theories of economists are interrelated in each phase of fiscal history. The author has a light touch throughout and does not attempt a technical treatment. The book is apparently addressed to the general reader and to the practitioner. Although there is reference to such matters of fiscal economics as the balanced budget multiplier, the author does not attempt to break new theoretical ground.

The great change that has occurred in this 170-year period lies in the role and responsibility of the national government for the aggregate level of income and employment. This change has necessitated an abandonment of rigid notions of budget-balancing and at least some diminution in fears about the horrors of the national debt. Kimmel's history of the emergence of what is commonly called "a positive fiscal policy" or "fiscal policy for growth and stability" points out that attitudes toward balanced budgets and the national debt had become stereotyped by the end of the 1920's. Adherence to fiscal orthodoxy was a traditional article of faith, and the march of events had not forced a rethinking of the issues. The attack on the conservative creed first came in the writings of economists, in the immediate pre-Keynesian period, from 1930 to 1936. The transformation in popular attitudes toward the national debt and deficits did not emerge until World War II, when it became completely evident that very large deficits and a rapid rise in the national debt were accompanied, not by bankruptcy and ruin, but by unprecedented increases in national output and an unprecedented rise in levels of material well-being.

Kimmel finds that, in the postwar period, the obsessive belief in the efficacy of a balanced budget has all but vanished, and that there are no important differences between the major political parties on this point. The philosophy of the Employment Act prevails, and budget policy will be adapted to the requirements of economic stabilization.

It is not possible to dissent from this conclusion. Certainly there is a predominant view that, in the event of a serious recession, federal taxes will be reduced and expenditures will be increased, let the deficits fall where they may. But there remains a curious lag in thinking in some quarters. Kimmel quotes a 1957 statement by Senator Goldwater, which appeared in the *Congressional Record*: "Where is the finely drawn line between freedom and slavery when, under the present deficit, every baby born in this country has a $1,675 first mortgage tag hanging around its neck?" Perhaps it is the senator who should read this book.

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Index Kewensis Plantarum Phanerogamarum. Supplement 12. George Taylor, Ed. Oxford University Press, New York, 1959. 162 pp. $12.

The appearance of a new supplement to the *Kew Index* has been eagerly anticipated by taxonomists who work with the flowering plants, for this continuing publication, perhaps more than any other single work, makes possible the orderly progress of plant taxonomy. The 12th supplement of the work (first published in 1895) covers the years 1951 to 1955 inclusive. An estimate indicates that between 17,000 and 18,000 binomials are included in this supplement; most of these are names of new species or new combinations published during the 5-year period, but some are names published earlier and overlooked in prior supplements. Like the other recent supplements, the 12th carries a separate listing of newly described (or previously overlooked) genera. There are 472 names in this list.

This is the first supplement that has appeared under the nominal editorship of George Taylor, the new director of the Royal Botanic Gardens at Kew. In his foreword Taylor gives credit to M. I. Skan for the preparation of the supplement. Botanists in all fields owe a debt of gratitude to both Skan and Taylor, and to the Royal Botanic Gardens, for the preparation and sponsorship of this indispensable work. In its format and in the excellence of its printing, the 12th supplement is a worthy addition to a distinguished series of botanical reference works.

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The Gifted Group at Mid-Life. Thirty-five years' follow-up of the superior child. vol. 5 of *Genetic Studies of Genius.* Lewis M. Terman and Melita H. Oden. Stanford University Press, Stanford, Calif., 1959. xiii + 187 pp. $4.50.

If we were to identify a large group of 11-year-olds with IQ's that place them within the highest 1 percent of the school population and follow their careers for 35 years, what would we find? Few biographers have the pertinacity to wait for an answer. It is our good fortune that Terman has undertaken such a study—and of no less than 1500 individuals. Unfortunately Terman's death occurred before he could witness the ground swell of interest in exceptional talent which followed the scientific events of 1957.

The book is concerned primarily with the status of the gifted subjects in 1955, when their average age was 44. Careful comparisons have been made of the incidence of death, suicide, mental illness, sexual maladjustment, schooling, occupational rank, income, and fecundity in the gifted group and in the general population. The authors have been remarkably successful in bringing together statistics that are scattered over many fields. The analysis shows that the gifted subjects continue to be superior in nearly every respect. We learn that the present birth rate of 2.4 children per mother among the gifted women and the wives of gifted men is insufficient to "maintain the

stock," although the authors point out that a number of child-bearing years remain. Of particular interest is the finding that the mean intelligence level as measured by the Concept Mastery Test significantly increased between the ages of 30 and 40.

For the reader who wishes to obtain a brief review of Terman's 35 years of research on gifted subjects, this will be a useful book. Readers of earlier volumes will find that it covers familiar ground. It provides a concise summary of an investigation which sought to tell us (i) what intellectually superior children are like as children; (ii) how well they turn out; and (iii) what some of the factors are that influence their later achievement. The last aim was only partially realized. The authors themselves conclude that the research has not yielded much information on methods and techniques for the education of the gifted. However, by showing the records of accomplishment of gifted children who go through our schools without any special educational opportunities, the authors have provided a valuable base line against which the effects of future innovations may be assessed.

DONALD L. THISTLETHWAITE
National Merit Scholarship Corporation, Evanston, Illinois

Mass Leisure. Eric Larrabee and Rolf Meyersohn, Eds. Free Press, Glencoe, Ill., 1958. x + 429 pp. $6.

This collection of readings roams widely over a rather loosely defined subject, but the serious reader should, in general, find it an informative and provocative set of skilled attempts to dissect and explain "the leisure that has become available, on an increasing scale, to the populations of the industrial West."

In confining their attention to the industrial Western nations, the authors point out a rather basic distinction between "class leisure" and "mass leisure," the former involving the use of free time which was secured as a right or prerogative of classes whose orientation was towards leisure, the latter involving the use of free time earned and paid for by people whose orientation is towards work. It is "the working class" that is now joining the ranks of the "leisure class" as a result of greatly reduced hours of work and a considerably increased span of life. But this new-found leisure has come faster than have the changes in our attitudes towards work and in our philosophies of life; thus, it has brought anxieties and "problems" along with the potential advantages of a freer and better life. The book is, in general, an explication of these social trends.

The volume begins rather pedantically by quoting the two pages on "leisure" found in the *Oxford-English Dictionary,* followed by the five pages on the same subject contained in the *Encyclopedia of the Social Sciences.* This may, unfortunately, prove too much for the general reader—a pity, because the readings become more interesting, meaty, and thought-provoking as the distance from this logical but unimaginative beginning increases. The succeeding sections, for example, contain a number of theoretical discussions of a philosophic, psychological, and sociological nature, such as Bertrand Russell's "In praise of idleness," Jean Piajet's "The explanation of play," Robert MacIver's "The great loneliness," and Margaret Mead's "The pattern of leisure in America." There is not much connection between these sections, but the reader will come away with a sense of expanded horizon and a greater depth of perception regarding the subject.

The third section of the book is devoted to empirical studies—statistics on the amounts and uses of leisure and field studies on leisure behavior. Lundberg's classic study of Westchester County, N.Y., is included, along with the *Fortune* report on "30 Billion for fun," and Cleveland Amory's description of American upper-class white society at Tuxedo Park appears alongside Franklin Frazier's account of the "black bourgeoisie."

Finally, in section four, the authors include a series of articles on specific forms of mass leisure, under the subheadings "Sports," "Hobbies," "Holiday travel," and "Fads and habits." Here will be found studies on "camping in the wilderness," the "do-it-yourself market," the "motivational pattern of drinking," and "sex as play."

The volume ends with some words about the future of leisure in our society and with a bibliography which "attempts to include all works on leisure written in the English language since 1900." The list is arranged by decade and includes only 22 items from the period 1900 to 1909 but over 400 items for the 8 years from 1950 to 1958—an indication of the growing importance of the subject to us today.

Although this is a professional rather than a popular book—in the sense, at least, that it is confined for the most part to serious items from the research and the theoretical literature—it is also a book about each of us who is struggling to adjust his way of life to make more profitable use of his leisure time, or at least to understand why he can't do so. Thus, I believe that any serious reader will come away from reading this volume with greater perspective on himself and on the people around him.

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Excavations at La Venta Tabasco, 1955. Philip Drucker, Robert F. Heizer, and Robert J. Squier. Smithsonian Institution, Washington, D.C., 1959 (order from Superintendent of Documents, GPO, Washington 25). viii + 312 pp. Illus. + plates. $4.

As the title implies, this monograph reports on extensive excavations made at the Olmec site of La Venta, Mexico, during approximately three months of 1955 by a National Geographic Society–Smithsonian Institution–University of California archeological expedition.

The purpose of these excavations was to obtain definitive information on patterns of construction or architecture—a problem concerning which previous excavations of the site had produced only inconclusive results. Most of the work was concentrated in the Ceremonial Court area (complex A), which lies just north of the pyramid. Four construction phases were recognized: phase 1, the water-sorted floors period; phase 2, the white-floor period; phase 3, the rose-floor period; and phase 4, the red-clay-cap period.

The authors are of the opinion that "La Venta represents a culturally 'floating' manifestation of classic (or florescent) Olmec culture which at the present time is not firmly anchored to other sites or culture periods either at its beginning or end" (page 259). Radiocarbon dates are interpreted as indicating that complex A was constructed and used continuously during, approximately, the period 800 to 400 B.C.

Two points of wider significance are discussed under the heading "Possible sociopolitical situation at La Venta." La Venta may have been selected as the district ceremonial center because of its relative isolation. Whereas the religious

centers of Europe are associated with large population centers, in Mesoamerica in Preclassic times there may have been a less intimate connection between economic factors, population numbers, and religion. The authors' assignment of the site to the Formative or Preclassic period of Mesoamerica runs contrary to "the opinion of some earlier writers who believed that elaborate systems of sociopolitical or socioreligious controls, as manifested by major construction projects, did not come into being until the Classic period" (page 269).

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Evolution by Natural Selection. Charles Darwin and Alfred Russell Wallace. Cambridge University Press, New York, 1958. viii + 288 pp. $4.75.

Among the spate of books published to celebrate the centenary of the publication of the *Origin of Species,* this volume occupies a special place. Over half of the book consists of an annotated and complete reprinting of Darwin's lengthy "Essay" of 1844, long virtually unavailable, in which he set forth his views about the causes and evidences of evolution with a comprehensiveness and a logical force that would have made writing of the *Origin* itself seem superfluous to a lesser man. This is the solid document Darwin entrusted to his wife with explicit written instructions for its publication in case he should die before completing the *Origin*.

Also included are Darwin's 47-page "Sketch" of 1842, an apparently unpolished but reasonably complete treatment of his theory; the famous Wallace-Darwin papers of 1858; a letter from Darwin to Asa Gray at Harvard; and, finally, an informative introduction to most of these documents, written in 1909 by Darwin's son Francis. Gavin de Beer has furnished an up-to-date and readable foreword.

For an American, special interest attaches to the Asa Gray letter, in which Darwin expounds and apologizes for his theory in, for him, an incredibly brief space. It is to be regretted that neither this book nor the recent *Darwin Reader* by M. Bates and P. S. Humphrey (Scribner, New York, 1956) includes the "P.S." at the end of this letter, for in it Darwin distinguishes between natural selection and the then unknown laws of what we now call mutations, which Darwin terms "all important" in providing the "groundwork for selection to act on."

No one can read these "essays" without being struck by the great importance Darwin gave to the relationship between the production of new breeds of domestic plants and animals by selective breeding and the evolution of new species by natural selection. The material on the evolution of animal behavior and instincts makes good reading now that Tinbergen and his school have reconferred scientific respectability on this important field. Because these essays preceded the publication of the *Origin* by a decade and a half, they serve as links between Darwin and his predecessors. Of them de Beer well says, "none of these precursors was able to compel attention, let alone adherence, to these views; and it is because of the completeness of his demonstration of the fact of evolution, and of the method by which it has been brought about, that the world owes its debt to Darwin."

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New Books

Anatomy and Physiology. vol. 1. Cells, tissues, integument; skeletal, muscular, and digestive systems; blood, lymph, and circulatory system (College Outline Series). Edwin B. Steen and Ashley Montagu. Barnes & Noble, New York, 1959. 347 pp. Paper, $2.50.

Annual Review of Plant Physiology. vol. 10. Leonard Machlis, Ed. Annual Reviews, Palo Alto, Calif., 1959. 490 pp. $7. Contents: "Prefatory chapter," W. J. Chandler; "Foliar absorption of mineral nutrients," S. H. Wittwer and F. G. Teubner; "Leaf proteins," N. W. Pirie; "Light-induced reactions of bacterial chromatophores and their relation to photosynthesis," A. W. Frenkel; "The structure of the chloroplast," J. J. Wolken; "Active transport of salt into plant tissue," G. G. Laties; "Respiratory mechanisms in higher plants," D. P. Hackett; "Photoperiodic control of floral induction," J. Doorenbos and S. J. Wellensiek; "The lignins," R. E. Kremers; "Fat metabolism in higher plants," P. K. Stumpf and C. Bradbeer; "The pine tree," N. T. Mirov and R. G. Stanley; "Physiology of virus diseases," F. C. Bawden; "Plant chemotherapy," A. E. Dimond and J. G. Horsfall; "The macronutrient elements," T. C. Broyer and P. R. Stout; "Nitrogen nutrition," R. H. Burris; "Metabolism of carbon compounds," M. Gibbs; "The chemical regulation of growth (some substances and extracts which induce growth and morphogenesis)," F. C. Steward and E. M. Shantz; "Auxins and fungi," H. E. Gruen; "Phototropism and phototaxis," J. Reinert.

The Chemistry and Technology of Cereals as Food and Feed. Samuel A. Matz, Ed. Avi Publishing, Westport, Conn., 1959. 747 pp.

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Evolution of Nervous Control from Primitive Organisms to Man. A symposium. Publ. No. 52. Allan D. Bass, Ed. AAAS, Washington 5, 1959. 238 pp. Members (prepaid), $5; nonmembers, $5.75.

Experiment Perilous. Physicians and patients facing the unknown. Renee C. Fox. Free Press, Glencoe, Ill., 1959. 262 pp. $5.

Families in Treatment. From the viewpoint of the patient, clinician, and the researcher. Erika Chance. Basic Books, New York, 1959. 252 pp. $5.50.

Family Planning, Sterility, and Population Growth. Ronald Freedman, Pascal K. Whelpton, Arthur A. Campbell. McGraw-Hill, New York, 1959. 526 pp. $9.50.

Optical Mineralogy. Paul F. Kerr. McGraw-Hill, New York, ed. 3, 1959. 456 pp. $8.50.

Pigment Cell Biology. Proceedings of the fourth conference on the biology of normal and atypical pigment cell growth. Myron Gordon, Ed. Academic Press, New York, 1959. 661 pp. $13.50.

A Primer of Programming for Digital Computers. Marshal H. Wrubel. McGraw-Hill, New York, 1959. 245 pp. $7.50.

Principles of Direct-Current Machines. Alexander S. Langsdorf. McGraw-Hill, New York, ed. 6, 1959. 380 pp. $8.50.

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Reports

Taxonomic "Descriptions"

Abstract. Original descriptions of organisms are often difficult to visualize, due to the fact that authors attempt to include the variability of the species in the description. Since the scientific name remains associated permanently with the holotype, it is suggested that the description of the holotype (which is a concrete thing) be segregated from the characterization of the species (which is conceptual).

My observations regarding the functions and objectivity of taxonomic "descriptions" result from experiences of the past 20 years in trying to visualize described organisms. I offer them in the hope that they may stimulate a discussion of methods which will result in less ambiguity in descriptions and, consequently, in greater ease of recognizing named forms.

Obviously the first questions to be answered are: What is a description? What is its purpose? *Webster's New Collegiate Dictionary* (ed. 5) defines *description* in the following terms: "Discourse, or an example of it, designed to describe a scene, person, emotion, etc." Since Webster utilizes *describe* to define *description*, it is necessary to refer to the former, for which the first part of the definition reads: "To represent by words." Webster gives as synonyms of *describe*: "represent, relate, recount, narrate, express, explain; depict, picture, delineate, characterize." The purpose of a description is to convey a concept of the object under scrutiny as clearly as possible by means of words, pictures, or diagrams.

But what are we describing? In the past, we have commonly stated that we are describing a "new species" or a "new genus." The result, in general, has been that the greater the amount of material the author had before him, the vaguer and less useful for identification his "description" became. Descriptions based upon long series become loaded with *more or less, usually, generally, about, a litte longer,* and comparable terms, with the result that a person trying to visualize the organism, or to match a specimen with the description, finds it extremely difficult to do so. Though such phrases or words are used deliberately because they are indefinite and ambiguous, they make it difficult for the reader to learn what the described specimens look like. Descriptions based upon uniques are usually more easily visualized than are those based upon series.

Are we, however, actually describing species or genera? At best we are describing only a very small segment of a variable and varying population which is represented by preserved specimens. It follows that any description or characterization of a "species" is necessarily imperfect, because no person knows the full extent of variability in any species. Furthermore, since no two specimens are exactly alike, the association of specimens must always be somewhat subjective.

Because of the impediments encountered in connection with verbal descriptions, the system of types was developed, the "holotype" being a single specimen selected by the author to represent the species permanently and to serve as a point of referral for authoritative information in case questions arise. Consequently, the closer the description comes to fitting the holotype exactly, the better the picture one can obtain of the typical specimen of the species.

Under such circumstances, would it not be best to follow the ensuing procedure in describing new species?

1) Describe the holotype exactly and in detail, giving comparative measurements in concrete terms or ratios. Selection of the holotype from the specimens available becomes the first step in "describing," if such a process is used. Since only a single specimen is involved, there can hardly be an excuse for ambiguity or vagueness in the description. Data regarding place and time of capture, collector, host, other pertinent information, and location of the holotype should be given. (A few authors do follow the procedure of describing the holotype at present.)

2) If it is available, describe the allotype and record data associated with it.

3) Attempt to characterize the species —that is, discuss the probable limits, variability, and geographical and host ranges of the species and the characters by which the species can be most easily recognized. Compare the remainder of the series—that is, the paratypes—with the holotype and explain your conception of the species.

4) Differentiate the species from others which have been described.

To a certain extent, a comparable *modus operandi* might be adopted for the higher categories, since each has its "type," but the descriptions of course become more and more inclusive.

Following such a procedure in describing new species would enable one to segregate the tangible (the holotype) from the intangible (the conception which the author has of the species) and would go far toward making the original description more useful to the person who has to refer to it. The procedure also makes justifiable the description of "species" upon the basis of uniques or small series (although this is not recommended unless a group is being revised or monographed).

The use of such a system would be one means of placing taxonomy on a more objective basis. It would certainly be of help in the future, as neotypes are designated to replace our present holotypes (which inevitably will be destroyed as the ages pass).

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26 February 1959

Instructions for preparing reports. Begin the report with an abstract of from 45 to 55 words. The abstract should *not* repeat phrases employed in the title. It should work with the title to give the reader a summary of the results presented in the report proper.

Type manuscripts double-spaced and submit one ribbon copy and one carbon copy.

Limit the report proper to the equivalent of 1200 words. This space includes that occupied by illustrative material as well as by the references and notes.

Limit illustrative material to *one* 2-column figure (that is, a figure whose width equals two columns of text) or to *one* 2-column table or to *two* 1-column illustrations, which may consist of two figures or two tables or one of each.

For further details see "Suggestions to Contributors" [*Science* **125**, 16 (1957)].

Bomb Carbon-14 in Human Beings

Abstract. The concentration of bomb-produced radiocarbon in human beings will lag behind the rising concentration in average atmospheric CO_2. Measurements on human materials suggest a lag of about 1 year for both breath CO_2 and blood, with the suggestion of a somewhat higher value for lung tissue. These results are in reasonable agreement with predictions based on independent evidence.

In evaluating the hazard to man of bomb-produced radiocarbon, one of the factors which must be considered is the time relationship between the C^{14} concentration in the carbon of the human body and that in the carbon of atmospheric CO_2. Several investigators (*1–3*) have published data on the atmospheric

radiocarbon increase resulting from nuclear tests. To our knowledge, no comparable information has been published for the human body.

To date three measurements of contemporary body radiocarbon have been made in our laboratory. The results given in Table 1 are expressed as permillage difference from the C^{14} concentration of a standard sample (*3*). In order to eliminate differences resulting from isotopic fractionation, C^{13}/C^{12} ratios were determined for each sample, allowing the radiocarbon results to be normalized to a common C^{13}/C^{12} ratio (see last column, Table 1).

The results are plotted in Fig. 1 along with the curve published by Broecker and Walton (*3*) for the C^{14} concentration in atmospheric CO_2 as a function of time. Since all the results are normalized to the same δC^{13} value, displacements from the curve reflect only a lag between the human materials and the average atmosphere. For blood and breath CO_2, this lag is 1.1 years; for lung tissue, 1.8 years.

Ideally, only two factors should contribute to this time lag: (i) the time between photosynthesis of food and consumption by human beings and (ii) the residence time of carbon in human tissue. However, if the curve of Fig. 1 does not represent the atmosphere in which foods eaten by the sample subjects were grown, this fact will also influence the lag times. As is discussed below, the true lag times are probably less than the values obtained from Fig. 1.

As pointed out by Suess (*4*) and Fergusson (*5*), plants growing in industrial areas show a greater fossil CO_2 effect (that is, lower C^{14} concentration) than those growing in areas removed from industrial activity. In addition, Munnich and Vogel (*2*) have suggested that plants growing in zones of dense vegetation may incorporate CO_2 given off by decaying organic materials in soils. Both effects will tend to lengthen the lag times obtained in Fig. 1, the curve of which is based on samples collected in areas remote from both industrial activity and dense vegetation.

Table 1. C^{14}/C^{12} ratios for human materials.

Sample	δC^{14}	δC^{13}	ΔC^{14}
Lung tissue, with associated blood, from a New York City resident. (Sample L-371A, June 1958)	61 ± 10	−14.7	42 ± 10
Respiratory CO_2 from a resident of Rockland County, N.Y. (Sample L-505A, 1 Jan. 1959)	106 ± 8	−21.8	105 ± 8
Blood from same person as sample L-505A. (Sample L-505B, 1 Jan. 1959)	115 ± 8	−16.5	102 ± 8

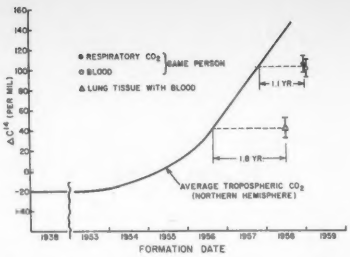

Fig. 1. Relationship between the normalized C^{14} concentration in human beings and the C^{14} concentration in tropospheric CO_2 of the Northern Hemisphere. Curve taken from Broecker and Walton (*3*).

Although it is not possible to predict how much too long the lag times might be, a few assumptions can give an idea about the magnitude. If the local "Suess effect" (dilution with industrial CO_2) were to be 1 percent greater than the world average value of 2 percent (*5*), the lag time as measured in Fig. 1 would be 0.2 year too long; this is based on the presently observed atmospheric increase of 5 percent per year. Furthermore, if plants were to photosynthesize a significant amount (say 10 percent) of decay CO_2 and if this CO_2 were derived mainly from organic material grown 3 years previously when the atmosphere was 15 percent lower in its C^{14} concentration, then an apparent lag of 0.3 year would be introduced. Thus it is evident that the lag times read from Fig. 1 are maximum values.

A crude estimate of the lag between photosynthesis and human consumption can be made by considering the average American diet. For fruits, vegetables and grains, the time is not likely to exceed 1 year, for the supply of these foods is almost completely replenished each growth season. Milk products should have a similar time lag, since the milk should closely reflect the animal diet (a cow produces its own body weight of milk in 3 months). An analogous situation exists for eggs. On the other hand, meat may show a longer lag, since the mean residence time of carbon in the animal must be taken into account. Since meat provides less than 20 percent of the carbon in the average diet, this effect is probably not sufficient to raise the average lag to more than 1 year.

To our knowledge, the biological half-life for carbon in the soft tissue and bone of man has not yet been determined. For soft tissue in rats the value is 35 days (*6*), but as was shown by Richmond and Langham (*7*), who have determined biological half-times of alkali metals in various mammals, the residence time for human beings may be considerably different from that for rats (for Cs^{137} it is 6.5 times longer). A lower limit for the turnover time can be obtained by dividing the carbon content of the body by the amount of carbon metabolized per day. The latter may be computed as food intake less fecal excretion or as respiratory CO_2 plus urinary carbon. In either case, the result is about 300 g of carbon. For a 150-lb person, therefore, the minimum turnover time for the body as a whole is about 40 days. This figure is also the length of time which respiratory CO_2 should lag behind ingested food.

From the foregoing information, these conclusions are warranted:

1) The interval between the fixation of carbon in average food and the consumption of that food is less than 1 year (that is, less than 1.1 years minus 40 days).

2) The maximum time which blood lags behind food is about 6 months; in other words, the mean residence time of a carbon atom in the blood is no more than 6 months. This is based on an upper limit (2 σ) for the difference between blood and respiratory CO_2 (20 per mill) converted to months through division by 4 per mill per month (equivalent to the slope of the atmospheric curve in Fig. 1) and supplemented by the 40-day lag between breath and food.

3) The carbon of lung tissue has a somewhat longer mean residence time than that of blood.

From this discussion it is evident that more measurements are needed. Among other things, further measurements should answer these three major questions: (i) To what extent does decay CO_2 enter growing plants? (ii) To what extent can variation in diet affect the lag time? (iii) What is the mean residence time of carbon in human soft tissue and in human bone? Answers to these questions will provide further data needed in assessing the hazard to man of bomb-produced radiocarbon (*8*).

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8. This work is sponsored by the U.S. Atomic Energy Commission (contract AF (30-1) 1656). We acknowledge the technical assistance of James Hubbard, Marylou Zickl, and Ross Horowitz. J. L. Kulp has provided help and encouragement for which we are appreciative. This report is Lamont contribution No. 352.

2 April 1959

Carrier State in Human Acatalasemia

Abstract. The heterozygous carrier state of a rare hereditary disease, acatalasemia, has been defined biochemically. Affected homozygotes have no blood catalase activity, whereas heterozygotes show activities intermediate between this inactivity and the activity of normal controls, without overlap. Pedigrees show a high frequency of consanguineous marriages.

In 1949 Takahara and Miyamoto (*1*) reported four siblings with progressive gangrenous gingivitis in whom catalase activity was not demonstrable in the peripheral blood. In this and subsequent communications (*2–4*), these authors described and elaborated upon the syndrome and coined the term *acatalasemia,* since the level of catalase activity in the tissues other than blood could not be ascertained.

At the time of our preliminary report (*5*), 38 cases of acatalasemia in 17 families had been documented by various investigators in Japan (*6*). In all but three families, two or more siblings were affected. A history of consanguineous marriage was obtained from 16 families. Among the parents of the acatalasemic individuals were ten instances of first-cousin marriages and two marriages involving not only first cousins but additional degrees of relationships as well; three sets of parents were more remotely related, and information on the others was uncertain. Takahara *et al.* (*3*) have suggested that the disorder is transmitted as a monogenetic recessive character; however, in these previous studies the genotypic characteristics were not investigated in the related family members in whom definite catalase activity has been demonstrated.

After review of the levels of catalase activity reported by Takahara (*4*), it occurred to one of us (E. T. N.) that the mean value of blood catalase activity among the positive catalase reactors of the acatalasemia families seemed to be lower than that noted for the control group. This was interpreted to indicate that certain members of the acatalasemia family probably had significantly low levels of catalase activity, while others had higher values, comparable to the control series.

To investigate this possibility (*7*), heparinized blood samples were obtained from five families previously studied by the Okayama University group. A modification of the method used by Herbert (*8*) was employed for the determination of blood catalase activity. The activity, which is expressed as the reaction velocity constant $K_{cat.}$ ($K_1 \times 10^3$), was determined for randomly selected Japanese subjects in Hiroshima and Nagasaki: the mean value was 5.54 (range, 3.91 to 7.10) in 206 individuals (*9*). In Fig. 1, of the 66 cases of positive catalase reactors in the five acatalasemia families examined, 30 had $K_{cat.}$ values which were definitely below those of the controls: the mean was 2.15 (range, 1.48 to 2.89), and there was no overlap with values for the controls. This phenomenon of low blood catalase activity is referred to below as *hypocatalasemia.* One can, apparently, with this biochemical technique, readily separate the hypocatalasemic from the normal and the acatalasemic states.

As Fig. 2 shows, certain members of the families of acatalasemic subjects are hypocatalasemic while others possess normal blood catalase values. It appears that (i) the parents of acatalasemic children are hypocatalasemic, (ii) the siblings of acatalasemic individuals may be either hypocatalasemic or normal, and (iii) an acatalasemic parent has hypocatalasemic children when mated to a normal individual. The possibility that an acatalasemic-hypocatalasemic parent combination may produce acatalasemic and hypocatalasemic children has not yet been demonstrated. Genetically, the hypocatalasemic state may be thought of as a carrier state for acatalasemia. Sawin and Glick (*10*) have described a similar genetic pattern for the enzyme atropine esterase, in rabbits. They ascribed the difference in the amount of enzyme activity to incomplete dominance—that is, ability to produce the enzyme is dominant over absence of this ability.

The results of the study reported here

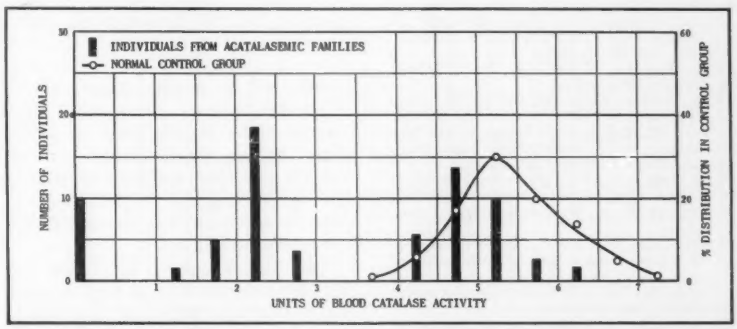
INDIVIDUALS FROM ACATALASEMIC FAMILIES
NORMAL CONTROL GROUP
NUMBER OF INDIVIDUALS
% DISTRIBUTION IN CONTROL GROUP
UNITS OF BLOOD CATALASE ACTIVITY

Fig. 1. Distribution of catalase values for members of the five acatalasemia families and comparison with a percentage distribution curve of values from a normal control group.

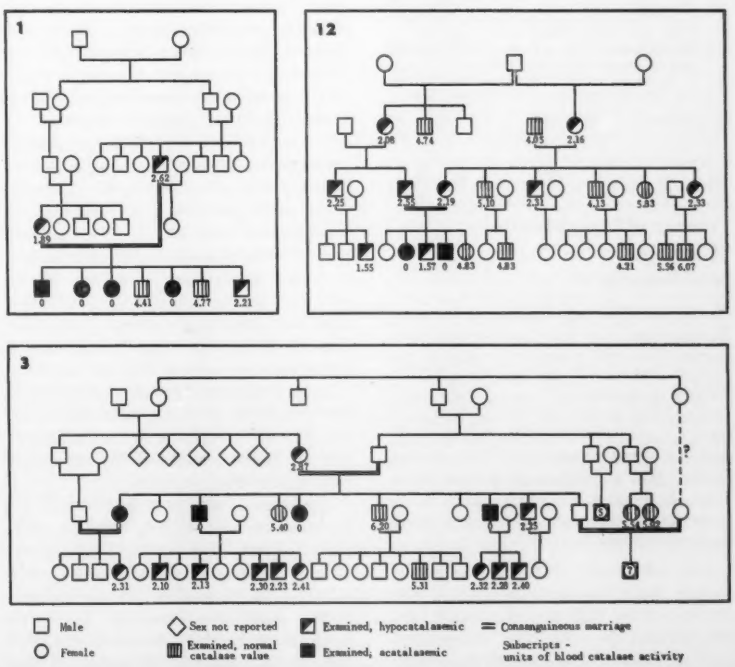

Fig. 2. Genealogical relationships in three of the acatalasema families, numbered according to Takahara (*6*).

resemble the data reported by these investigators and suggest that in this respect the underlying genetic phenomenon may be similar. The exact nature of the defect in acatalasemia is at present not clear.

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7. These data were collected under the auspices of (i) the Atomic Bomb Casualty Commission, a field research agency of the U.S. National Academy of Sciences–National Research Council, with financial support from the U.S. Atomic Energy Commission, administered in cooperation with the National Institute of Health of the Japanese Ministry of Health and Welfare, and (ii) the Department of Otorhinolaryngology of Okayama University Medical School, Okayama, Japan.
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20 April 1959

Presence of Myoglobin in "Cartilage" of the Marine Snail Busycon

Abstract. The odontophore of the snail *Busycon* has been found to contain myoglobin. The red pigment is readily extracted with water, and spectrophotometric analysis shows the characteristic peaks of myoglobin at 575 and 539 mμ. Although the odontophore is considered to be cartilaginous, it contains, not a chondroitin sulfate, but a polyhexose sulfate. This unusual chemical composition may be responsible for the absorption of myoglobin by the odontophore.

The occurrence of myoglobin in the marine snail *Busycon caniculatum* was first noted by Ball and Meyerhof (*1*), who studied the myoglobin and respiratory enzymes of the muscles associated with the radula of this snail. Since the blood pigment of *Busycon* is hemocyanin, it is of interest to note the occurrence of an additional oxygen-carrying pigment. In recent studies on the cartilage-like tissues of some marine invertebrates it was noted that the odontophore (the supporting rod for the radula in the proboscis) of *Busycon* contained a bright red pigment. This pigment was readily extracted with water, and subsequent analysis of the aqueous solution of pigment in a Beckman model DU spectrophotometer proved it to be myoglobin. This solution of pigment gave an α peak at 574 mμ and a β peak at 538 to 539 mμ. These peaks are in agreement with the data of Ball and Meyerhof (*1*) for the muscle myoglobin of *Busycon*.

Myoglobins have a higher oxygen-carrying capacity and are more soluble than hemocyanin. Therefore, myoglobin would probably be a more efficient respiratory pigment in the comparatively active movements of the radular apparatus. The radular musculature is rich in myoglobin, and so is the odontophore, to which the muscles are attached. The fact that the odontophores of young snails do not contain myoglobin and that those of older snails do accumulate the pigment suggests that myoglobin is stored there and is not essential to the metabolism of the tissue.

The odontophores of snails are frequently thought of as being cartilage tissue. With the advent of biochemical analysis, cartilage is currently defined as tissue which contains one or more of the chondroitin sulfates and which, upon hydrolysis, yields a hexosamine (D-galactosamine), a uronic acid, and sulfuric acid. Alkaline extracts of the odontophore extracted with potassium chloride and potassium carbonate (*2*) yielded a material which, upon ionophoresis (borate buffer, *p*H 10.0; potential gradient, 18 v/cm), produced a spot which stained metachromatically with alcoholic thionin (0.15 percent in 65-percent ethanol). This metachromatic material had an ionophoretic mobility (−12.2) slightly greater than that of chondroitin sulfate (−11.8). Pronounced streaking of the material during ionophoresis indicated that the extracted material was badly degraded. On improving the methods of extraction by use of trypsin digestion (*3*), an extract was obtained which gave no indication of being degraded. This extract was strongly metachromatic and had an ionophoretic mobility similar to that of the alkaline extract.

Upon acid hydrolysis (4.0*N* HCl for 18 hours or 1.0*N* H_2SO_4 for 4 hours in sealed glass tubes) and subsequent chromatography (butanol:acetic acid:water, 3:2:1), neither hexosamine nor uronic acid was detected. The only hydrolyzate product detected was glucose. Sulfate analysis (*4*) indicated that the extract contained ester sulfate. The polysaccharide of the odontophore is therefore a polyglucose sulfate and not one of the mucopolysaccharides normally found in cartilage (*5*). Since the odontophore does not contain the polysaccharides characteristic of cartilage, it cannot be considered normal cartilage. A further analysis of the apparently unique chemical composition of the odontophore may help to explain why the myoglobin is absorbed by the chondroid matrix (*6*).

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5. A more detailed report on the identification of the polyglucose sulfate is in preparation.
6. This work was done while I was a Lalor Foundation fellow at Marine Biological Laboratory, Woods Hole, Mass.
* Helen Hay Whitney fellow.

27 March 1959

Notes on the Champlain Sea Episode in the St. Lawrence Lowlands, Quebec

Abstract. Palynological studies, coupled with geological investigations and radiocarbon dating, have shown that the Champlain Sea episode in the St. Lawrence lowlands is in part contemporaneous with the Two Creeks interstadial of the Wisconsin glaciation.

Recent studies made on Pleistocene deposits of the St. Lawrence lowlands, Quebec, involving stratigraphic studies by Gadd (*1*), Karrow (*2*), and McClintock (*3*), and palynological studies by Potzger (*4*), Potzger and Courtemanche (*5*), and me (*6*) have clarified the chronology of the late Pleistocene events in that area enough to warrant a reassessment of the previously accepted sequence of these events.

The palynological studies (Fig. 1) have indicated conclusively the regional presence of the postglacial pine period (Fig. 1, pollen zones III and IV), the hypsithermal interval (*7*), and the earlier spruce maximum (Fig. 1, pollen zone V) in post-Champlain Sea deposits of the St. Lawrence lowlands.

In sediments older than those showing the spruce maximum (zone V) the higher percentages of pine pollen (*Pinus banksiana*), accompanied by an increase of non-tree pollen, have been interpreted as evidence for a late-glacial episode (zone VI). The high percentages of pine pollen should, perhaps, be explained by over-representation due to the high pol-

len production of pine. This effect becomes more apparent when the total amount of tree pollen from local sources is low.

The late-glacial episode was followed by a change toward a colder and moister climate, or both (*8*), as indicated by the marked increase of spruce (zone V). This increase of spruce, often accompanied by an increase of balsam fir, cannot be explained as a local development in the forest succession because in all diagrams this spruce maximum has a definite age and position, preceded and followed by other regionally consistent maxima and minima in other pollen graphs. Hence this spruce maximum must be caused by a regional climatic change and can be used as a chronological marker in the pollen stratigraphy.

The pine zone (Fig. 1, zones III and IV) consists of an early part (zone IV), characterized by pollen of jack pine (*Pinus banksiana*), and a main part (zone III) when pollen of white pine (*Pinus strobus*) and red pine (*Pinus resinosa*) were predominant. The main part of the pine pollen zone (zone III) is believed to be contemporaneous with the postglacial hypsithermal interval (*7*). An age of 5000 to 7000 years for this interval seems generally acceptable.

The spruce maximum (zone V) is necessarily older and may correlate with the upper part of the spruce zone in Connecticut, dated at 8155 ± 410 years (radiocarbon sample Y-282). Since the pollen record in the St. Lawrence lowlands extends somewhat further back in time, an age of 8500 to 9000 years, based on palynological and paleobotanical reasoning, seems conceivable for the earliest postglacial pollen-bearing sediments. It was most encouraging when a radiocarbon age of 9430 ± 250 years (sample L 441-C) was obtained for the basal sediments of the St. Germain bog near Drummondville (*6*, p. 5).

Since the pollen diagrams showing pollen zones V and VI at the base are from the center of the St. Lawrence lowlands, only about 300 feet above present sea level, it follows that the main part of the Champlain Sea episode must be an older event; the maximum limit of the marine inundation of the area reached the present elevation of 600 feet.

Furthermore, the studies by Gadd (*1*) and Karrow (*2*) have conclusively proved that, contrary to the interpretation by Osborn (*9, 10*), the St. Narcisse moraine a few miles north of the St. Lawrence River at Three-Rivers, Quebec, was formed by ice from the north and the readvance was contemporaneous with a part of the Champlain Sea episode.

In this connection it should perhaps be mentioned that recent mapping and study of Pleistocene deposits in the lowlands have failed to support Osborn's speculation about a local ice lobe in the lowlands (*9, 11*) and, as also shown by Gadd (*1*), I have been able to prove to my satisfaction that the "marine crevasse fillings" in the Lotbiniere area (*9*) are longitudinal and parabolic dunes formed on the crest of the Drummondville moraine (*1*) in early post-Champlain Sea time. The sand is clearly wind-transported and contains no fossils.

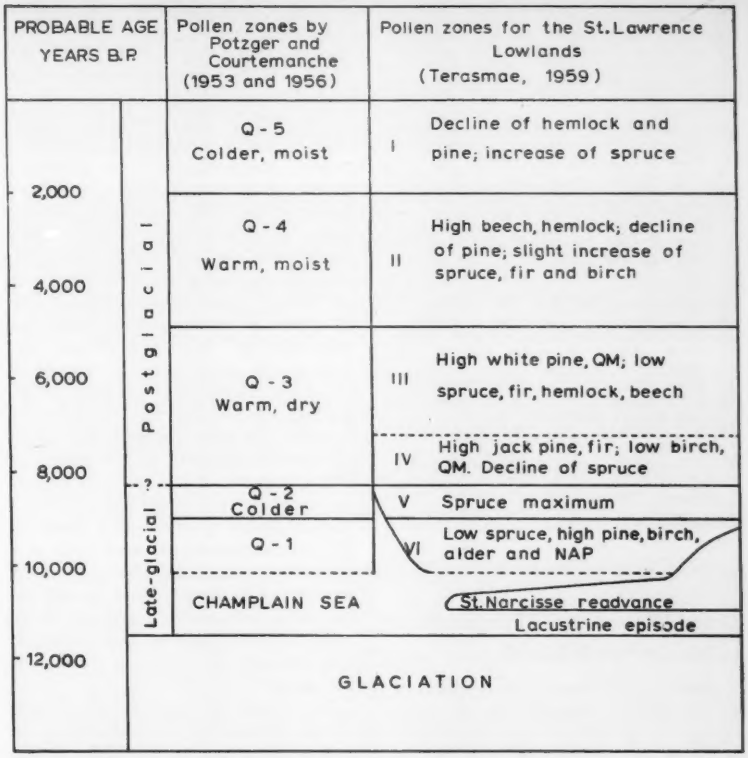

Fig. 1. Pollen zones and late Wisconsin events in the St. Lawrence lowlands (QM, *quercetum mixtum*; NAP, nonarboreal pollen).

Some years ago samples of marine shells, embedded in deposits of the Champlain Sea, were collected from sites near the highest marine limit in the Ottawa and Montreal area, by Elson, Prest and Wagner of the Geological Survey of Canada and these samples were dated by the radiocarbon method at the Yale Geochronometric Laboratory (*12*). The following dates (*13*) were obtained: 10,630 ± 330 years (Y-215), 10,850 ± 330 years (Y-216) and 11,370 ± 360 years (Y-233). On grounds of possible contamination by old carbonates and the fact that "they were incompatible with the dated Great Lakes sequence," these dates were discarded by Flint (*12*, p. 278) as "probably spurious." Since these dates are consistent among themselves, it is my opinion that they may be of the right order of magnitude and thus approximately date an early phase of the Champlain Sea episode. This view is shared by H. de Vries of the Groningen radiocarbon laboratory, Holland, who accompanied me on field trips to the St. Lawrence lowlands last summer. These dates would be, indeed, difficult to explain if the time limits generally suggested for the Champlain Sea episode (from about 5000 to 7000 years ago) were correct (*12*, p. 272).

The present evidence indicates that the Champlain Sea episode in the St. Lawrence lowlands may be in part contemporaneous with the Two Creeks interval and hence the St. Narcisse readvance can perhaps be correlated with the Valders (?) substage. The palynological studies, supported by radiocarbon dates, suggest that pollen zone V, showing the spruce maximum, may indicate a later readvance of ice in northern Quebec and probably at Cochrane, Ontario. If so, this readvance should have occurred about 7500 to 8000 years ago. It is interesting to note that the sequence of events as outlined above is in accord with the results of studies made by Fairbridge (*14*) on the Late-Pleistocene eustatic sea-level fluctuations.

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Growth Responses of Phycomyces to Polarized Light Stimuli

Abstract. The effectiveness of stimuli in air depends upon the direction of polarization. For sporangiophores immersed in a medium of refractive index similar to that of the protoplasm this dependence disappears. These facts indicate that one is dealing with simple Fresnel reflection losses and not with dichroism of oriented photoreceptors.

For the tropic response of *Phycomyces* sporangiophores, Castle (*1*) observed that light polarized horizontally (electric vector perpendicular to the long axis of the sporangiophore) was 10 to 15 percent more effective than equal incident flux polarized vertically. Castle concluded that the differential response to polarized light was due to Fresnel reflection losses at the front surface of the cylindrical sporangiophore. Reflection losses are larger for vertically polarized light. Recently, however, Jaffe (*2*) stated, without explanation, that differential reflection cannot explain the observation.

Table 1. Average values of intensity (arbitrary units) necessary for growth response null. "Polarized vertically" denotes that the electric vector was parallel to the long axis of the sporangiophore; 15 to 18 determinations were averaged for each 450 mμ value and only 7 and 4 for 380 mμ value. Standard errors of the mean are given.

Direction	In air ($\lambda = 450$ mμ)	In air ($\lambda = 380$ mμ)	In FC-43 ($\lambda = 450$ mμ)
Polarized vertically	1.24 ± 0.02	1.21 ± 0.01	1.06 ± 0.04
Polarized horizontally	1.00 ± 0.03	1.00 ± 0.06	1.00 ± 0.05

He suggested that since the photoreceptors of *Phycomyces* are likely to be dichroic, the differential response might be due to an alignment of photoreceptors within the sporangiophores.

To test this hypothesis the relative effectiveness of horizontally and vertically polarized light in producing growth responses was measured by a null method. The apparatus, intensity units, and growth response measurements have been described previously (*3*). Sporangiophores were rotated continuously (2 rev/min). A standard blue stimulus of fixed intensity impinging bilaterally 60° from the vertical long axis of the sporangiophore was alternated every 5 minutes with a test stimulus impinging unilaterally 90° from the vertical. The monochromatic test stimulus (wavelength 450 or 380 mμ) was plane polarized by a glass laminated linear polarizer (Polaroid Corp., HN22). The dispersing elements of a Beckman DU spectrophotometer were used as a monochromator, and the intensity of the test beam was controlled by varying the slit width. Intensity as a function of slit width was calibrated against a thermopile standard with an RCA 935 phototube-amplifier system.

The increase in sporangiophore length was measured to the nearest micron for each 5-minute period. The difference in growth during two consecutive 5-minute stimuli (blue followed by test) was ideally zero if the test stimulus matched the blue stimulus. If the test signal was too large, a growth response maximum occurred during the blue stimulus (response occurs 5 minutes after stimulus) and a positive difference, blue minus test, was observed. If it was too small, a minimum occurred during the blue, and a negative difference was observed. For a given intensity and polarization of the test stimulus the sum of the differences for three cycles was plotted against log slit width. The intensity necessary for a growth response null was determined graphically.

In air (Table 1), the horizontally polarized beam is about 20 percent more effective than the vertically polarized beam, as would be expected from Castle's observation for the tropic response. The growth response has the advantage of requiring no assumptions about the spatial distribution of the light within the sporangiophore in producing a response, except that the light must enter to be effective.

If the observed effect were due to dichroism of oriented receptors, the magnitude of the effect might be expected to be different for a second strong absorption band. However, the response differences for the two directions of polarization at two action spectrum maxima, 380 and 450 mμ (*4*), were identical.

To eliminate reflection losses, sporangiophores were stimulated while immersed in totally fluorinated tributylamine (FC-43, Minnesota Mining and Manufacturing Co.). The FC-43 has an index of refraction at 25°C of 1.29, which is close to the average value for intact sporangiophores (1.38) (*5*), and is optically inactive. Thus, the reflection losses are negligible, since they are a function of the square of the difference in index of refraction at the interface. In FC-43, growth rates are normal and a usual time course of response to a large pulse-up is observed. Since there is little focusing effect, continuous rotation of the sporangiophores was unnecessary. In FC-43 (Table 1) there is no significant difference in growth response for the two directions of polarization.

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18 March 1959

On the Instrumental Conditioned Reaction Evoked by Electrical Stimulation of the Hypothalamus

Abstract. Weak electrical stimulation of the hypothalamic lateral area of satiated goats elicited the previously established conditioned reflex of putting the left foreleg on the food tray and then eating the food given as reinforcement. When, during stimulation, food was not given, an extinction of the conditioned reaction took place.

Experiments on alimentary conditioned reflexes performed several years ago by one of us (*1*) led to the conclusion that an already established instrumental reaction is due to the excitation of a hypothetical "alimentary center" caused by the conditioned stimulus. The discovery of the feeding center in the lateral hypothalamic area (*2*) has suggested that it may correspond to our hypothetical center. Therefore, to test our conclusion, we performed experiments with electrical stimulation of the hypothalamic feeding center of satiated animals.

An instrumental conditioned reflex of putting the left foreleg on the food tray was established in four goats; every movement was reinforced by a small amount of oats. After a period of training, the hungry animal, on entering the

experimental pen, started immediately to perform the learnt movement; after some minutes of repeated conditioned reactions, the movements became less frequent and then ceased to appear. A kymogram of a usual experiment is shown in Fig. 1*A*. If the learnt movements were not reinforced by food, the usual course of extinction of the conditioned reflex was observed: at first the movements grew more frequent and energetic, then they gradually disappeared; sometimes the movements returned for a while, then disappeared definitely (Fig. 1*C*). When the food was given again the reflex was immediately restored.

After the training series an experiment with electrical stimulation was performed in each goat. According to Hess' method (*3*) adapted to goats by Andersson (*4*), three unipolar electrodes were unilaterally placed in the lateral hypothalamic area of the animal under local anesthesia. Immediately after the operation the animal was placed in the experimental pen and, usually after several minutes, it began to perform the learnt movement and eat food. After some minutes the goat, completely satiated, ceased to perform the movements and did not eat the offered food. Then the electrical stimulation (0.5 to 1.5 volt; 10 to 50 pulses per second) was applied.

At first the animal usually showed a kind of orientation-examination reflex; then licking frequently appeared, and afterward (10 seconds to 1 minute or more after the beginning of the stimulation) the animal came to the food tray and performed the learnt movement which was reinforced by food. If the stimulation lasted several minutes the movements were repeated and food was eaten as in the usual experiment; this is shown in Fig. 1*B*. After the interruption of stimulation, one or two movements still appeared sometimes; the animal, however, refused to eat the food, remained for a while with the hoof on the food-tray, then, as if very surprised, it went slowly away.

In three goats an extinction of the conditioned reflex was performed in such a way that the food was not given for some minutes during the electrical stimulation. As is seen on the kymogram of this experiment (Fig. 1*D*) the course of extinction is very similar to that occurring under normal conditions (Fig. 1*C*). At the beginning the learnt movements become more frequent and then, after about 1 minute, they disappear; several minutes later a few movements appear again, then the animal goes away from the food tray and, despite continuing stimulation, does not show any interest in the food. After about 7 minutes the goat was called to come to the food tray and the food was given. After eating the food the goat began to repeat the learnt movement which was now every time reinforced by oats. A similar result was obtained in two other goats.

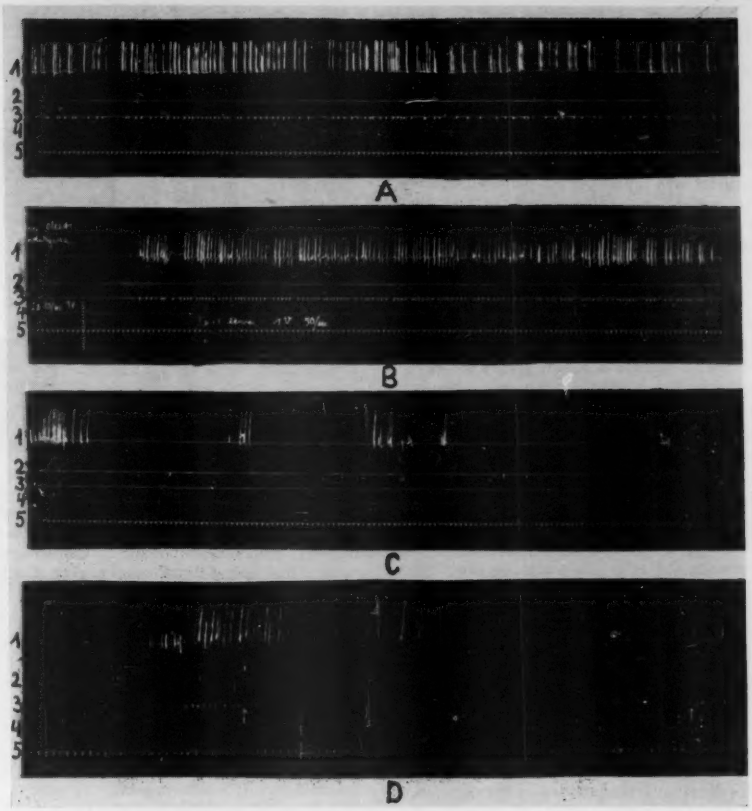

Fig. 1. Kymograms of experiments with goat No. 5. *A*, Course of a usual training experiment. *B*, Experiment with electrical stimulation of the lateral hypothalamic area. *C*, Extinction of the conditioned reflex under usual conditions. *D*, Extinction of the conditioned reflex during electrical stimulation. 1, Learnt movements of foreleg; 2, (not pertinent to this study); 3, food given; 4, electrical stimulation; 5, time (5 seconds). The arrows in *C* and *D* show the period in which the food was not given.

When the experiment lasted several hours and the animal was more and more satiated with food, the movements evoked by stimulation were less and less frequent and only a part of food offered was eaten every time.

The animal was killed the same day and its brain was removed for anatomical control.

The elicitation of the conditioned instrumental reaction by electrical stimulation in these experiments is in harmony with the results obtained by other investigators (*5*) and confirms our previous conclusion, mentioned at the beginning of this report. Some points of our results ought, however, to be explained. The performance of one or two learnt movements after the interruption of stimulation seems to be caused by inertia of the conditioned movement's center. The similar course of extinction in usual conditions and during the stimulation seems to indicate that in both cases some inhibitory agents intervene; they are perhaps connected with the lack of impulses deriving from the reinforcement.

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14 January 1959

Isolation of Several Peptides with Relaxin Activity

Abstract. When a relaxin preparation was subjected to counter-current distribution for 355 transfers in a butanol–trichloroacetic acid system, significant biological activity was found over a band appreciably wider than was expected for a single component. By refractionation of the active band, three peptides with relaxin activity were isolated.

We have previously reported (*1*) that when aqueous ovarian extracts are distributed between 2-butanol and 1.0 percent trichloroacetic acid (TCA) the distribution of relaxin activity corresponds to that expected for a single molecular species, with $K = 2.7$ to 3.0. It was subsequently discovered that the system 2-butanol/0.2 percent TCA afforded results more favorable for purposes of purification, since the partition coefficient for relaxin in the latter system lay between 0.7 and 0.9, while the bulk of inactive material concentrated in the upper phase. More extensive experiments with relaxin in the 2-butanol/0.2 percent TCA system indicated the existence of more than one substance with relaxin activity. The isolation and partial purification of three active peptides is described in this report.

Relaxin was prepared from acetone-dried sow ovarian powder by the method of Frieden and Layman (*2*). Nine and seventy-five hundredths grams of a preparation containing 40 GPU/mg was distributed in separatory funnels between 2-butanol and 0.2 percent TCA; the volume of each phase was 250 ml. Eleven transfers were applied. The material in funnels 4 to 8, which included most of the activity and 17 percent of the total starting material (as measured spectrophotometrically at 285 mμ) was recovered and distributed between the same solvents in the Craig apparatus for 100 transfers. More than 80 percent of the original activity was located in tubes 36 to 60. This fraction was recovered and redistributed as before. After 105 transfers the contents of tubes 0 to 34 and 66 to 99 were removed and replaced by fresh solvents, and the distribution was continued for another 250 transfers with the apparatus arranged for recycling. The concentration of solute in each tube was measured spectrophotometrically, and relaxin activity was determined in pooled aliquots from 11 or 12 tubes over a band 67 tubes wide.

The results are shown in the upper

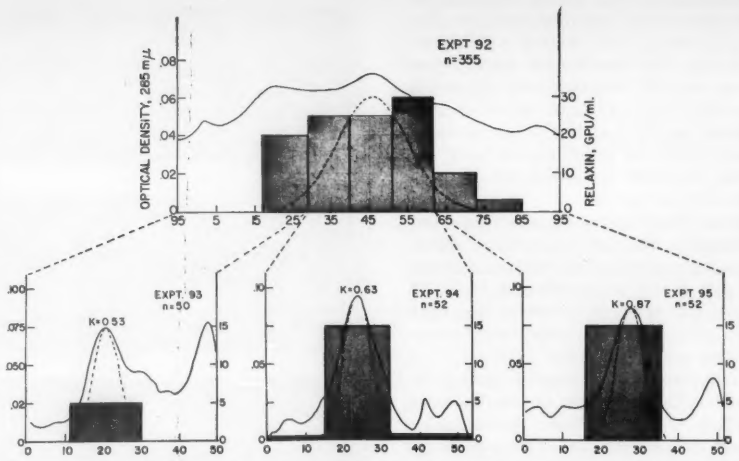

Fig. 1. (Top) Distribution of relaxin for 355 transfers in the system 2-butanol 0.2 percent TCA. (Bottom) Distribution of material pooled as indicated from experiment 92 for 50 to 52 transfers in the same system. Solid lines indicate optical density at 285 mμ (left ordinate); shaded bars indicate relaxin concentrations (right ordinate). The broken lines represent theoretical distribution curves. Abscissa: tube number.

half of Fig. 1. The solid line represents the solute concentration (left ordinate), and the shaded bars indicate relaxin concentration (right ordinate). The broken line is the distribution calculated for a substance with a distribution coefficient of 0.7, corresponding to the peak observed at tube No. 45. It may be observed that significant relaxin activity is found over a band appreciably wider than would be expected for a single substance.

The material from experiment No. 92 was pooled as indicated (tubes 29 to 61, 62 to 94, and 95 to 28); each fraction was concentrated, the excess TCA was removed by dialysis, and the recovered material was rerun in the same system for 50 to 52 transfers. Three peaks with relaxin activity were identified, with K's of 0.53, 0.63, and 0.87, respectively (lower half of Fig. 1). As before, relaxin concentrations are indicated by shaded bars, calculated distributions by broken lines. Peptide A ($K = 0.53$) was contaminated with a faster moving component; the configurations of peaks B and C ($K = 0.63$ and 0.87, respectively) were such as to indicate that each might contain up to 25 percent of material other than the main component. Quantitatively, the material recovered in the three active peaks totaled about 1 percent of the weight of the original preparation.

Paper chromatography of acid hydrolyzates of the three active peaks indicated the presence in each of the following amino acids: Asp, Ala, Arg, $(CyS)_2$, Glu, Gly, His, Lys, Ser, Thre, Val, and two leucines. From hydrolyzates of the DNP-derivatives of the intact-peptides, only ε-DNP lysine could be identified. All of the relaxin-active peptides which have been isolated exhibit an absorption spectrum characterized by a broad, nearly flat inflection in the region 255 to 285 mμ, ($E^{270}_{1\%} = 5.5$ to 7.5). The reason for this is unknown, since neither tyrosine nor phenylalanine could be detected in their acid hydrolyzates, and less than 0.5 percent tryptophan was found in the intact peptides by the method of Spies and Chambers (*3*, *4*).

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27 February 1959

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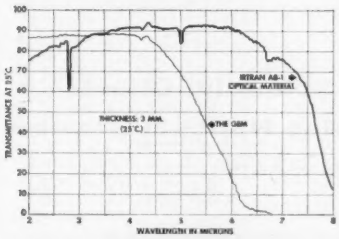

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Acido orotico

Around the turn of the century it was brought to the world's attention that the inhabitants of certain Bulgarian villages were a) living to ripe old ages and b) consuming vast quantities of the ripe old fermentation products of the local dairying. Echoes of this coincidence have rumbled forth at intervals since.

In the twenties a certain elderly biochemist who had seen much importance in the correlation was a celebrated figure of Paris. In the thirties American milk wagons were bedizened with signs advertising a certain brand of fermented milk. In the forties the word "yogurt" entered the vocabulary of the American intelligentsia. With the dawn of the fifties, the Journal of the American Chemical Society (*72*, 2312) reported that certain strains of *Lactobacillus bulgaricus* throve when supplied with 6-carboxyuracil, a substance first synthesized in 1897 for academic exercise and later shown to be identical with orotic acid. This name was derived from ορος, whey, by two Italians who had encountered the substance while making lactose from milk whey liquors.

The flowering of biochemical sophistication in the mid-fifties has excited a deeper curiosity about orotic acid. To some it looks like a significant intermediate in the process by which living organisms fabricate nucleotides for their DNA—the stuff of genes—out of the amino acids at their disposal. This is big talk.

In Italy interest in *acido orotico* has been rekindled to a small-scale frenzy. At the University of Urbino last June a colloquium on pyrimidines (*Acta Vitaminologica*, *12*, 195-328) devoted much of its attention to the compound. One man claimed his evidence showed that a dietary deficiency of orotic acid affects pregnancy, lactation, and growth in the rat, that it is a vitamin-like factor essential for the survival of the newborn. One senses the closing of a circle.

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Meetings

Contamination of the Moon

A committee to consider the implications of contamination of the moon and planets by extraterrestrial exploration (CETEX) was established by the International Council of Scientific Unions (ICSU) in March 1958 and held its first meeting two months later in The Hague. On the basis of two days of discussions, CETEX decided that there is a real possibility that exploration experiments could contaminate the moon or the planets in such a way that other experiments, particularly biological, would be made impossible. The dangers of such contaminations, and steps to avoid them, were pointed out in the report of CETEX [*Science* **128**, 887 (1958)] which was accepted by ICSU at its general meeting in Washington in October 1958. In addition, the parent organization asked CETEX to meet for a second time, with the help of appropriate technical experts, to draw up a code of conduct for space research and to suggest the correct sequence of experiments of different types.

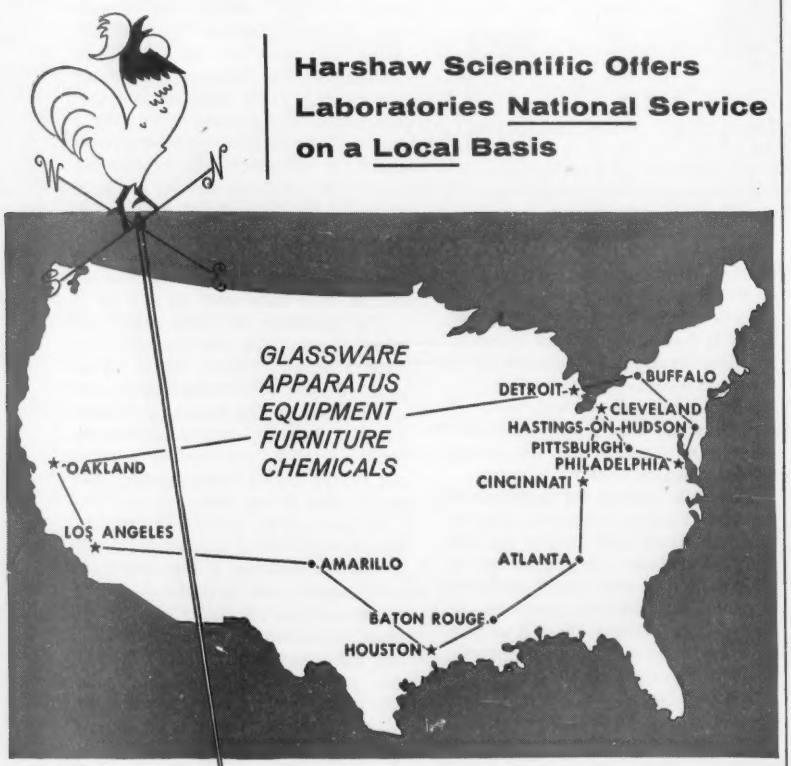
The second meeting was held at The Hague 9–10 March 1959, the following members being present: M. Florkin, convener (Belgium); P. Alexander (Great Britain); J. Bartels (Germany); W. O. Fenn (U.S.); D. J. Hughes (U.S.); J. Roche (France); and J. Rösch (France).

It was impossible for CETEX to prepare a detailed sequence of space experiments, principally because of the short time interval between its two meetings and the complexity of the problems involved. Instead, it drew up some general principles governing space exploration that could serve as suggestions to COSPAR, the newly organized space research committee of ICSU, which presumably will include the work that CETEX has begun. These general principles are given below. Although the first report of CETEX was considered in detail at the second meeting, primarily only minor changes of a technical nature were made in it. The committee reaffirmed its position that the presence of life of any type on the moon is extremely unlikely and decided, as well, that the possibility that free radicals in explosive amounts exist on the moon is remote. A suggestion was added to the report that methods for sterilization of rockets be developed as rapidly as possible, and that sterilization be instituted as a standard procedure.

The general principles governing space research drawn up at the meeting are as follows.

"1) Space research offers a challenge and opportunities which should appeal to the most imaginative minds. The greatest encouragement must be given to novel and unconventional approaches and no proposal should be sanctioned which would hamper the experimenters' freedom of action unless there are compelling reasons. On the other hand, equally imaginative thinking is required when considering possible complications which can follow a particular type of experiment. Surprises are certain and unlikely possibilities must be borne in mind when dealing with the problem of contamination, which is better defined as the problem of reducing the risk whereby one experiment may spoil the situation for other subsequent enquiries. The question of deciding whether such a conflict is likely to arise can best be dealt with by a committee or working group engaged in planning, or advising on scientific experiments.

"2) Ideally scientists should be asked to inform COSPAR as early as possible of each space experiment which is envisaged and of the methods to be used in its execution. The broadly based committee of COSPAR containing scientists



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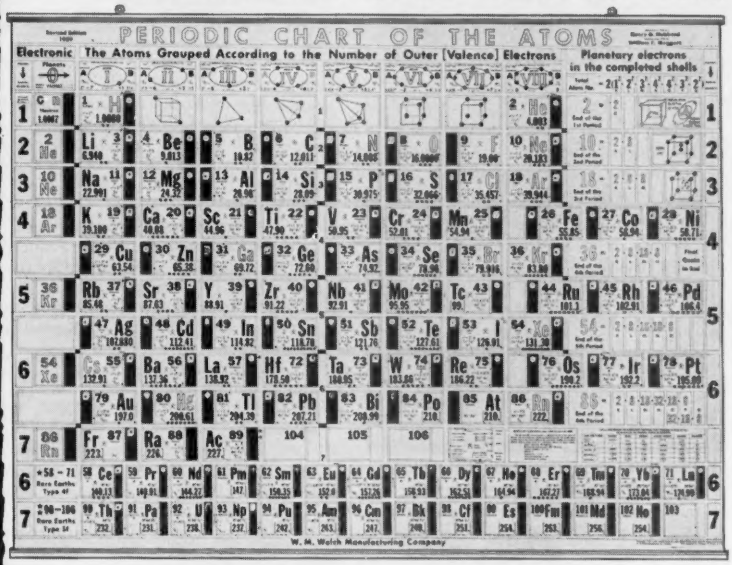
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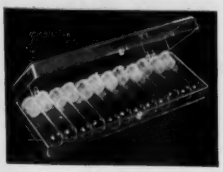

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from all disciplines may be able to see much more clearly than the space research specialists possible conflicts introduced by such experiments and may be able to suggest ways of overcoming these difficulties.

"3) There are a number of obvious and necessary experiments which are bound to be done and here the COSPAR working group dealing with experiments may be able to suggest priorities. While it may not be possible to avoid all types of contamination a proper sequence can ensure that the collection of data is not thereby hindered. For example CETEX recommends positively that no 'soft' landing, which requires the release of large quantities of gases, should be made on the moon until experiments have been successfully carried out—or at least all reasonable attempts made—to determine the nature of the moon's atmosphere.

"4) In view of the great uncertainties which face space research all operations which are not capable of conveying meaningful scientific data are to be discouraged even if they do not appear to carry with them a known source of contamination. Risks with the unexpected must be taken as otherwise no space exploration is possible but such risks must be justified by the scientific content of the experiment."

DONALD J. HUGHES
Brookhaven National Laboratory, Upton, Long Island, New York

Forthcoming Events

September

1–3. Association for Computing Machinery, natl., Cambridge, Mass. (J. Moshman, Council for Economic and Industry Research, Inc., 1200 Jefferson Davis Highway, Arlington 2, Va.)

1–6. College of American Pathologists, Chicago, Ill. (A. H. Dearing, Suite 2115 Prudential Plaza, Chicago 1.)

1–7. History and Philosophy of Science (General Assembly, History Div., Intern. Union of the History and Philosophy of Science), Barcelona, Spain. (R. Taton, IUHPS, 64, rue Gay-Lussac, Paris 5e.)

1–8. Acoustics, 3rd intern. cong., Stuttgart, Germany. (E. Zwicker, Breitscheidstrasse 3, Stuttgart N.)

1–7 Oct. International Civil Aviation Organization (Meteorological Div.), Montreal, Canada. (ICAO, Maison de l'Aviation Internationale, Montreal.)

2–4. Allergy, 4th European cong., London, England. (British Assoc. of Allergists, Wright-Fleming Inst., St. Mary's Hospital, London, W.2.)

2–4. Cryogenic Engineering Conf., Berkeley, Calif. (K. D. Timmerhaus, CEC, Chemical Engineering Dept., Univ. of Colorado, Boulder.)

2–4. Crystal Imperfections and the Chemical Reactivity of Solids (Faraday discussion), Kingston, Ontario, Canada. (Faraday Soc., 6 Gray's Inn Sq., London, W.C.1, England.)

2–5. American Mathematical Soc. and Mathematical Assoc. of America (joint summer), Salt Lake City, Utah. (E. Pitcher, AMS, Lehigh Univ., Bethlehem, Pa.)

2–8. Foundations of Mathematics: Infinitistic Methods, symp., Warsaw, Poland. (A. Mostowski, Dept. of Mathematics, Univ. of California, Berkeley 4.)

2–9. British Assoc. for the Advancement of Science, 121st annual, York, England. (Secretary, BAAS, 18 Adam St., Adelphi, London, W.C.2, England.)

3–4. Magnesium in Agriculture, symp., Morgantown, W. Va. (D. J. Horvath, Dept. of Animal Husbandry, West Virginia Univ., Morgantown.)

3–5. Nephrology, 1st intern. cong., Geneva, Switzerland, and Evian, France. (G. Richet, Hospital Necker, 149, rue de Sevres, Paris 7e, France.)

3–6. American Sociological Soc., natl., Chicago, Ill. (D. Young, Russell Sage Foundation, New York 22.)

3–9. American Psychological Assoc., annual conv., Cincinnati, Ohio. (R. W. Russell, APA, 1333 16 St., NW, Washington 6.)

4–7. International Federation of Surveyors, annual (by invitation), Gracow, Australia. (IFS, 4, Kanaalweg, Delft, Netherlands.)

5–12. Application of Radiation Sources in Industry, intern. conf., Warsaw, Poland. (P. Fent, IAEA, Vienna, Austria.)

6–12. Standards on a Common Language for Machine Searching and Translation, intern. conf., Cleveland, Ohio. (Secretariat, Center for Documentation and Communication Research, Western Research Univ., Cleveland 6.)

6–12. World Confederation for Physiotherapy, 3rd intern. cong., Paris, France. (A. Nicolle and J. Dupuis-Deltor, Société d'Organisation des Congrès Français et Internationaux, 1, rue Chanez, Paris 16e.)

7–9. Psychometric Soc., Cincinnati, Ohio. (P. H. DuBois, Washington Univ., St. Louis 5, Mo.)

7–9. Society of General Physiologists, Urbana, Ill. (F. G. Sherman, Dept. of Biology, Brown Univ., Providence 12.)

7–10. Institute of Management Sciences, Paris, France. (A. S. Manne, Dept. of Economics, Yale Univ., New Haven, Conn.)

7–11. American Soc. of Clinical Pathologists, Chicago, Ill. (C. E. Wells, 2052 N. Orleans, Chicago 14.)

7–11. Illuminating Engineering Soc., annual natl. conf., San Francisco, Calif. (A. D. Hinckley, IES,, 1860 Broadway, New York 36.)

7–12. European Soc. of Haematology, cong., London, England. (E. Neumark, Dept. of Pathology, St. Mary's Hospital, London, W.2.)

7–12. World Medical Assoc., 13th general assembly, Montreal, Canada. (WMA, 10 Columbus Circle, New York 19.)

8–15. Sociology, 4th world cong., Milan and Stresa, Italy. (Intern. Sociological Assoc., Skepper House, 13 Endsleigh St., London, W.C.1, England.)

9–10. Air Pollution, 2nd intern. cong., New York, N.Y. (American Soc. for Mechanical Engineers, 29 W. 39 St., New York 18.)

(See issue of 19 June for comprehensive list)

New Products

The information reported here is obtained from manufacturers and from other sources considered to be reliable, and it reflects the claims of the manufacturer or other source. Neither Science *nor the writer assumes responsibility for the accuracy of the information. A coupon for use in making inquiries concerning the items listed appears on page 350.*

■ IONIZATION GAGE achieves linearity over a wide operating range by enclosure of the ionization region with a grid-like end covering that prevents straying of ions. Range of the gage for accurate measurements is said to be 10^{-10} mm-Hg with direct readings possible down to 10^{-12} mm-Hg. Flash filament technique permits estimation of partial pressures of adsorbable gases to 10^{-12} mm-Hg. Full-scale ranges from 0-to-10^{-3} to 0-to-10^{-10} mm-Hg are selectable. (N. R. C. Equipment Corp., Dept. 954)

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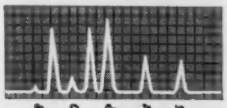

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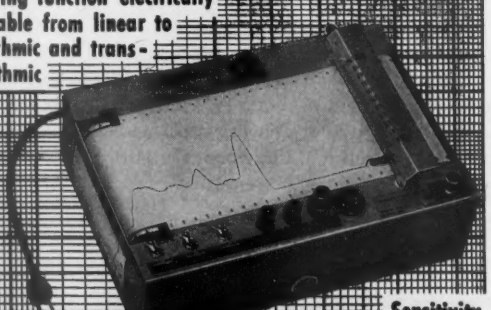

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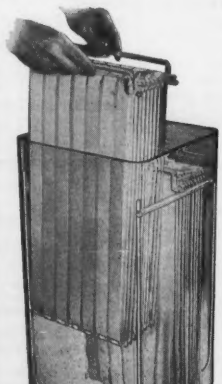

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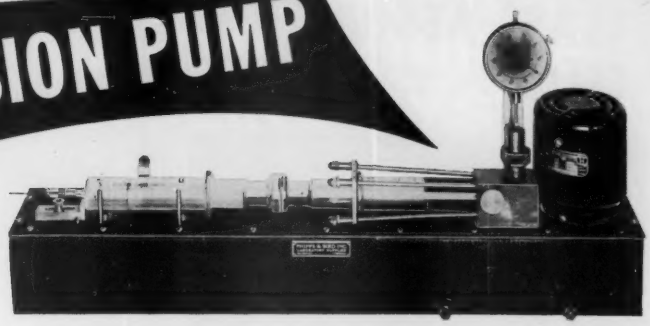

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City..... Zone..... County..... State.....

Age..... ☐ Single ☐ Married ☐ Male ☐ Female

Occupation.....

Location of Car (if different from residence address).....

Is car principally kept and used on a farm or ranch?.....

Yr.	Make	Model (Dlx., etc.)	Cyl.	Body Style	Purchase Date Mo. Yr.	☐ New ☐ Used

1. (a) Days per week car driven to work?..... One way distance is.....miles.
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2. Additional male drivers under age 25 in household at present time:

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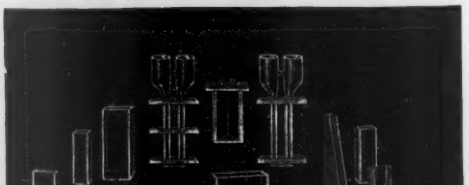

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Strehler, B. L. and Totter, J. R.; *Methods of Biochemical Analysis*, Vol. I, page 341. Editor, David Glick, Interscience Publishers, Inc., New York, 1954.

McElroy, W. D. and Strehler, B. L.; *Arch. Biochem.* 22, 420, 1949.

Strehler, B. L. and Totter, J. R.; *Arch. Biochem. Biophys.* 40, 28, 1952.

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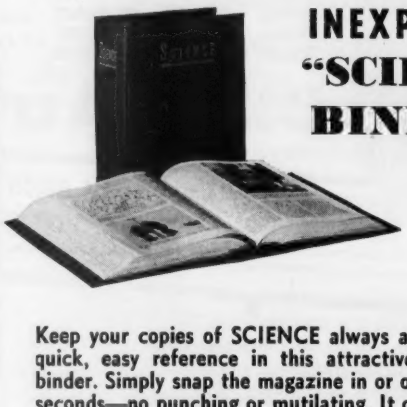

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Letters

(*Continued from page 298*)

made after removal by filtration of almost all living plant and animal cells and larger particulate matter. We were interested in whether the bound polyphosphates on clay micelles can be released and used by phytoplankton, but we had difficulty with analyses of soluble phosphorus and did not get all the data desired. Several analyses showed, however, that soluble phosphorus was almost nil, indicating the complete removal of this form by plankton. Furthermore, we did have several short-period (rather than minor) blooms of phytoplankton which almost certainly required more than minimal quantities of phosphorus.

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We have a longer paper in preparation which gives more complete data.

L. A. WHITFORD
RONALD C. PHILLIPS
North Carolina State College
School of Agriculture, Raleigh

Professions

The letter entitled "What is a profession?" [*Science* **129**, 1688 (1959)] prompts me to send you the definition credited to Justice Brandeis: "A profession is an occupation requiring preliminary *intellectual* training, pursued primarily for others and not merely oneself, and accepting as the measure of achievement one's contribution to society rather than individual financial reward."

A. R. PATTON
Colorado State University, Fort Collins

W. W. Benton, in his letter "What is a profession?," states, "Only three of these require certification, namely, medicine, education and, *in some states,* engineering" (italics mine).

This statement is incorrect, as all our 50 states and Puerto Rico now require registration for an engineer to practice. This, of course, means independently and for a consideration. Thus, an engineer can practice his profession without being registered if this is done under the supervision of a registered professional engineer, who assumes the responsibility for such engineering work.

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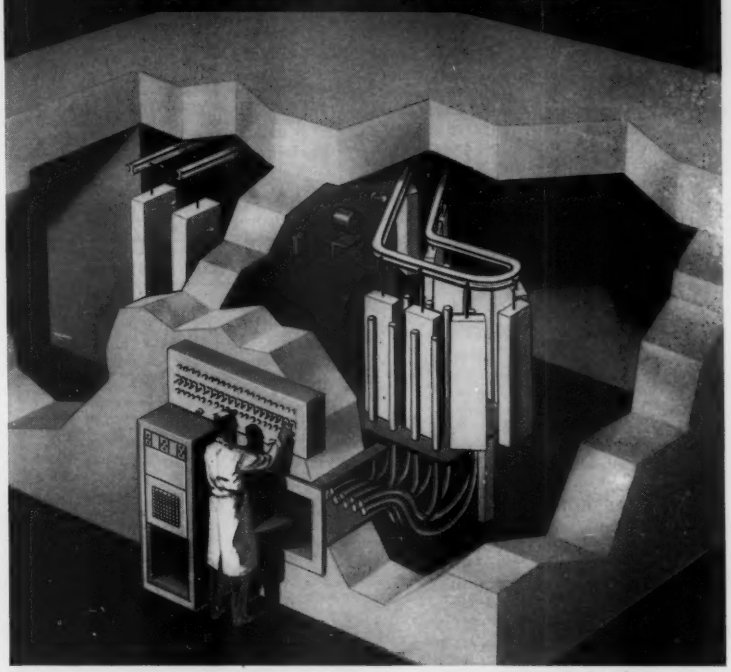

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Goddard Space Flight Center

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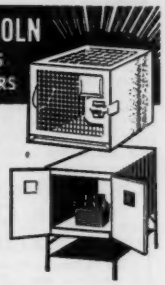

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3rd printing March 1959

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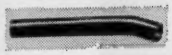

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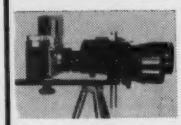

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Cat. No.	Size	Application	Overall dimensions	Cover	Window	Feeder	Price *Each
1105.	12	General Purpose, for mice, rats, hamsters, guinea pigs, etc.	11 in. long × 8½ in. wide × 6 in. high	A	with	round 23 cu. in.	6.50
1105-B.	12	do.	do.	A	without	do.	6.50
1105-D.	12	do.	do.	B	with	V-shape 53 cu. in.	9.65
1105-F.	12	do.	do.	B	without	do.	9.65
1105-H.	22	Mouse Cage, in accordance with U. S. N.I.H. Spec. FG-84	11½ in. long × 7½ in. wide × 5 in. high	A	with	round 23 cu. in.	6.50
1105-J.	22	do.	do.	A	without	do.	6.50
1105-L.	22	do.	do.	B	with	V-shape 46 cu. in.	9.65
1105-N.	22	do.	do.	B	without	do.	9.65
1105-P.	32	Breeding Cage for mice; also for rats, etc.	19 in. long × 10½ in. wide × 5⅛ in. high	B	with	V-shape 63 cu. in.	13.00
1105-R.	32	do.	do.	B	without	do.	13.00

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